

Where will the Calories come from?

IN the past few months, there has been a growing sense of public anxiety about future sources of energy. In the United States, the issue has been dramatized by the possibility that the United States and the Soviet Union will reach agreement on the common exploitation of the natural gas fields of Siberia (see page 249) and by the prospect that, with the Texas oil wells running at full capacity for the first time in two decades, the United States will soon have to buy petroleum from the Middle East. Britain is to some extent sheltered from the strains which have affected other industrial nations by the laggard growth of the economy in the past few years, but even the Central Electricity Generating Board has recently become apprehensive at the rise in oil prices. On the mainland of Europe the pressures are still more acute. And everywhere it seems that the prospect that nuclear energy will soon provide substantial supplements to conventional methods of generating electricity has been delayed and even dulled by events as varied as unexpected construction difficulties, problems of development and problems of finding sites. So is there a danger that the fuel crisis which has been predicted in the past few months will soon become a reality? And what will be done if it arrives?

The first need is to distinguish between long-term problems and the more immediate causes of discontent which have fanned the recent talk of crisis. Especially in the United States, the present and the future are too easily confused. The immediate cause of discontent in the United States is not that petroleum reserves are about to run out but that it will soon be necessary to import petroleum, most probably from the Middle East. Proved reserves in North America (excluding the North Slope of Alaska) amount to just about ten years supply at the present rate of consumption, and there is no particular reason to think that these will be substantially increased within the United States itself except by drilling in the deep waters of the continental shelf. But dependence on imported oil will have all kinds of disadvantages. The political hazards cause most anxiety, just as they did in Britain in the 1950s, but they can probably be lived with. In the long run, the United States will most probably have to pay more, possibly much more, for petroleum than it does at present and the consequences will be considerable although not catastrophic. After all, there are few who would seriously consider that the United States would grind to a halt if the price of crude petroleum were doubled from its present cost of just over \$2.50 a barrel, and even the prospect that the OPEC countries might get three or four times as much for what they sell need cause no abrupt change in the pattern of life and industry in North America. Even the prospect that imported oil might cost two or three per cent of the GNP in foreign exchange—a gloomy estimate—is not insupportable.

Petroleum, after all, is not the only source of energy or even of liquid fuel. The present sense of crisis has been heightened by the way in which the normal process

of substitution of one material by another has been hampered by all kinds of troubles. Strip mining runs into trouble from the environmentalists (but may be made more palatable if only the price of energy as a whole increases to the point at which mining companies can be compelled to spend more of their revenues on restoration). In the same way, the nuclear plants which might by now have contributed somewhat more to energy production have been unnecessarily impeded by environmental anxieties (which is not to say that the Joint Committee on Atomic Energy is not doing a public service by promising a thorough re-examination of the safety of nuclear reactors). And there is every reason to think that in the next ten years or so quite conventional research and development could help enormously to win gas and other chemicals from underground deposits of coal or even from exotic materials such as sewage.

But are these devices not just palliatives? That is what the crisis lobby says. And there is of course a sense in which they serve only to postpone the point at which petroleum is so costly that it will be used primarily as a chemical feedstock. What happens then? The first thing to be said is that nobody can predict with confidence what will happen to the pattern of demand for energy now that the United States seems on the brink of a dear-fuel economy. It would be a great surprise, however, if the consumption of crude petroleum in the United States—which at present consumes a quarter of the world's production—doubles in the coming decade as it did in the 1960s. By the same token, there is every chance that the consumption of electricity in the United States and elsewhere will not in future grow at the pace established in the past few decades. Those who belong to the "limits to growth" school, and who are constantly saying that the only question is to know which of several unwelcome and even bizarre forces will eventually decelerate the pace of growth have their answer in this example—the price mechanism is a powerful restraint.

None of this implies that energy consumption will not be larger, in the United States and other industrial nations just as in the world as a whole, a decade or more from now. But there is every prospect that the changing pattern of the energy industries will somehow manage to supply the energy the industrialised economies need. The price at which this will be done is the factor that will, as always, strike the balance between supply and demand. Yet in the long run, it is clear that there must come a time when dependence on organic fossil fuels must be attenuated, which is why so much turns on the prospects for nuclear energy, fusion as well as fission. This is why there needs to be a more deliberate programme of research and development on fast reactors and on the problems with the management of radioactive wastes. Is it too much to hope that the threat of an energy crisis will persuade the United States that these are projects that might with advantage be tackled internationally? At present, too much energy and effort is



frittered away on needless duplication. And there is a case for a continuing reassessment of the prospects for fusion reactors. In the long run, these must provide the best hope of a continuing supply of energy. After the first few heady moments in the 1960s, the prospect of controlled fusion has seemed—as it must—a distant goal. But success in half a century would be a comfortable outcome of the work now under way—an interval roughly comparable with the time taken to turn Faraday's discoveries in electromagnetism into workable generators of electricity in bulk.

Insuring for Thalidomide

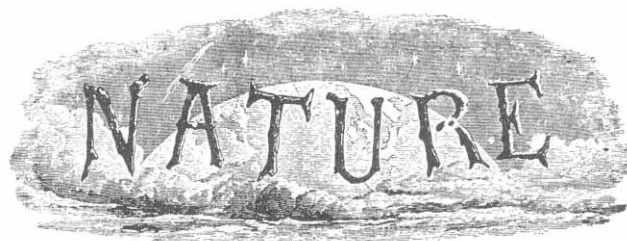
THE thalidomide disaster in the early 1960s casts a long shadow, as the debate in the House of Commons this week has shown clearly enough. And nobody will complain that members of all parties have for several weeks been exercised at the long delay which there has been in settling the civil legal suits by means of which the victims of this disaster—parents as well as limbless children—have sought compensation from the manufacturers of the drug in Britain, the Distillers Company Ltd. Although monetary compensation is only an inadequate recompense for the damage that has been done, it is obviously intolerable that it should have been so long delayed. And when all the fuss is over, there will be an urgent need to re-examine the principles on which the High Court has forbidden the publication in the *Sunday Times* of articles which might be held to increase the pressure from the public to see that compensation is as generous as possible. But there is no suggestion that the delays in settling these civil suits could have been avoided, and there is no assurance that with present arrangements for assessing the legal liability of pharmaceutical manufacturers, the consequences of further accidents of this kind will not be equally unpalatable. So is there not a case for a recasting of the arrangements for determining the liability of manufacturers for accidents which they perpetrate?

The issue is important not merely so that justice shall be quickly done but also because there is a danger that the present muddle about compensation is likely to be a powerful restraint on the willingness of pharmaceutical companies to put new drugs on the market. To be sure, when thalidomide was first launched, there was nothing to suggest that it might be teratogenic and no formal procedures—in Britain at least—for screening drugs for such effects. Even now, however, there remains a chance that some new product might turn out to be capable of causing damage as widespread as that now attributed to thalidomide, for no screening process based on animal experiments can ever be entirely cast-iron. And the risk that some technical innovation will cause unexpected damage is not confined to the pharmaceutical industry. What would happen, and who would be responsible, if there were a latter-day version of the disaster that struck Halifax, Nova Scotia, during the First World War when an ammunition ship in the harbour exploded?

In present circumstances, at least in Britain, most of the technical innovations that might lead to widespread and unexpected damage involve the government, directly or implicitly. Drugs must now be approved before being put on the market. Industrial plants of all kinds cannot

be put just where their designers wish, but must satisfy all kinds of planning requirements. So is it proper that, if something should unexpectedly go wrong, the government (as custodian of the public interest) should have its hands quite free? The case of nuclear energy is a good illustration of how governments have formally accepted responsibility as insurers of the last resort, and the International Convention on third-party liability, now ten years old, is a splendid model that might be followed in quite different fields. The notion is that the operators of nuclear plants are responsible for insuring against the costs of accidents up to a certain very large sum of money—\$50 million—but that governments step in thereafter to carry the larger risks that might well be involved. Especially because, in theory at least, nuclear accidents could have international consequences, this arrangement has some obvious political advantages. But the principle that could usefully be applied elsewhere is that the governments implicitly responsible for agreeing that particular technical innovations should go forward will shoulder ultimate responsibility for insurance against accidents of unexpected seriousness, even though those directly responsible for innovations should, alone, take the first substantial slice of responsibility.

100 Years Ago



The Great Meteoric Shower

As you will most probably have received from many other correspondents a general description of the magnificent spectacle on last Wednesday evening, I will confine my remarks principally to those observations which bear directly on the most important point at issue, viz., whether this meteor stream can be identified with the well-known comet of Biela. Having searched, during the autumn, on every available occasion for a glimpse at the approaching comet, and the almost unvarying cloudiness of the early morning sky having rendered even the negative value of the observations well-nigh useless, I read with delight the prediction of Dr. Weiss, and felt the greatest interest in its fulfilment.

Immediately I had noticed that a meteoric shower was in progress on the evening of the 27th, I directed the two assistants of the observatory, who have had considerable experience in tracing the paths of meteors during the last few years, to devote their whole attention to the accurate determination of the radiant point. In the meantime, with the assistance of three of the students of the philosophy class and of the meteorological assistants of the observatory, I noted the rate per minute, the velocity, direction, magnitude, &c., of the falling bodies.

The Radiant was found to be on the line joining γ and δ Andromedæ, and twice as far from δ as from γ . This gives as the R.A., $26^{\circ} 37'$, and N. Decl. $43^{\circ} 48'$, agreeing very well with the prediction.

The Epoch is somewhat in advance of that predicted; but this cannot be wondered at, as the comet has not been seen since 1852, and, in three complete revolutions round its orbit, it could scarcely have been expected not to have been subjected to considerable unknown perturbation, either from planets whose masses are imperfectly known, or perhaps from some neighbouring meteor-stream.

S. J. PERRY

Stonyhurst Observatory, Dec. 1

From *Nature*, 7, 84, December 5, 1872.

OLD WORLD

Lord Zuckerman Defends His Position

LORD ZUCKERMAN launched a counter attack on the scientific community this week, defending his policy of parsimony in cancer research (see *Nature*, **240**, 4; 1972). At a meeting of the British Association for Cancer Research, Lord Zuckerman claimed that because of the narrow mindedness of the scientific community, the "galvanizing ideas" of less well-established scientists may be wasted.

Members of the association attending this meeting were visibly self-conscious in the face of the recent massive cancer campaign launched in the United States. But if there is to be no loosening of purse strings on this side of the Atlantic, at least it is clear that British cancer research is likely to benefit from the recent improvement in the organization of existing facilities. Thanks to the MRC Coordinating Committee on Cancer Research, represented at the meeting by its chairman, Lord Rosenheim, unnecessary duplication of research is now being eliminated. The emergence of oncology as a discipline in its own right, moreover, is signalled by the institution of two academic chairs in clinical oncology, financed by the Imperial Cancer Research Fund and the Cancer Research Campaign at St Bartholomews and the Royal Marsden hospitals respectively. Lord Nathan, of the Cancer Research Campaign, announced that five more chairs carrying life tenure would be established on funds from the CRC sometime in the future.

Of the estimated £6 million in public funds spent annually on cancer research, some will now be spent on pilot oncological centres attached to four hospitals. These centres are to be financed jointly by the MRC and the Department of Health and Social Security.

One of the expected advantages of these regional centres is that they will encourage greater understanding and collaboration between basic scientists and clinicians. They will also promote the training of and provide the experience for "career scientists" specifically in oncology. Both Lord Rosenheim and Professor E. Scowen (ICRF) insisted on the need for career scientists in oncology.

As to the direction in which such hypothetical career scientists should be pointed, Professor Sir David Smithers (Royal Marsden Hospital) rose to the attack of the subcellular approach.

Supracellular biologists, he maintained, have made a far greater contribution to the progress of cancer therapy than molecular biologists. In this connexion, Mr Benno Schmidt, of the US National Program for the Conquest of Cancer, confessed that the \$51 million program of virus research in the United States is now "under the supervision of a panel" which is to decide whether it has reached a point of diminishing returns. The silence from the molecular biologists present at the meeting was deafening.

Lord Zuckerman said that money "obviously will buy brains",—what it

will not buy, however, is original ideas. Lord Zuckerman, it seems, has been receiving large numbers of letters since his report on funding of cancer research was published at the end of October. These letters are from people who have had difficulty in getting their original ideas taken seriously by the oncological establishment. Lord Zuckerman refused to commit himself on the value of their ideas which, he said, he was not qualified to judge. But someone has to. With limited funds, selection must necessarily be rigorous and Lord Zuckerman can undoubtedly expect many more letters.

EUROPEAN TECHNOLOGY

How to Succeed

from a Correspondent

THE basic problems associated with the development of an integrated European research and development programme are essentially those of decision making at the management level. This was the basis of Professor Hermann Bondi's Stevenson Memorial Lecture delivered at the London School of Economics last week.

Management is essentially the science of human relationships, Professor Bondi said, and he illustrated the necessity of delegation by governments by contrasting the smooth progress of the European Space Research Organization (ESRO) with the ill-fated Europa Launcher Development Organization (ELDO). No rocketry is involved in ESRO (the satellites are launched by an American carrier), and the governments involved are prepared to delegate management and planning decisions to the ESRO organization itself. But although ELDO is based on a liquid-fuel rocket (which is of virtually no military use), it does involve concepts of "defence", so there has been no delegation and the organization is supposed to be run by a secretariat which has no executive powers. The contracts are divided among participant countries on the basis both of task-sharing and of cost-sharing, and this has resulted in "perfect machinery for escalation".

The arguments in favour of international cooperation are well known; less proliferation of advanced products and research institutions means better staffing of those institutions which remain. The difficulties, said Professor

Bondi, are basically those of human relationships and applied psychology. Practical matters such as the principle of fair returns (that is, that a participant country should receive contracts roughly in proportion to its investment in the project) can in fact be solved fairly easily—by awarding more contracts to international consortia, so that the co-operation in fact takes place at the industrial level and not merely at the governmental level.

The real difficulty arises with international decision making. The scientific advisers to any government, said Professor Bondi, are liable to be appointed on a fairly *ad hoc* basis. Decision making is often a matter of choice between two almost equally suitable alternatives, and what matters is that a decision should be made swiftly rather than the nature of the decision itself. If, however, the advisers of one country suggest one course of action while those of another suggest a different one, "national policies" may be formed, even though there may be little to choose between the two proposals on objective grounds, and valuable time may be lost. The answer is clearly that there should be an international scientific advisory body for such projects.

Other problems of a practical nature—the differing trends in industrial development in the member countries, the necessity for "slimming down" duplicated effort, and of deciding which establishments are to be sacrificed in the process, are seen by Professor Bondi as a necessity painful to national interests that can only be solved without undue anguish by the companies concerned cooperating and producing an integrated European technology.

ENVIRONMENT

Birds and Pollution

A CALL for more basic research on pesticide metabolism in the body and for standardization of measurements of pesticide residue was made last week at a conference held under the auspices of the British Ornithologists Union, the British Trust for Ornithology and the Royal Society for the Protection of Birds.

Sir Frank Fraser Darling and Dr Duncan Poore, joint chairmen of the conference, both emphasized the importance of birds as sensitive indicators of environmental conditions, which made their study even more important at a time when man's various activities encroach more and more on the environment. This theme was expanded by Dr Robert Risebrough of the Canadian Wildlife Service, who said that coastal and marine birds seem particularly vulnerable to interference by the persistent organochlorines, which include DDT and its derivatives, and the polychlorinated biphenyls (PCBs).

DDT and PCBs, said Dr Risebrough, are now distributed throughout the world and small amounts of DDT have been found in penguins and in ocean-going birds in areas far from direct sources of pollution. The amounts of DDT and PCBs found in the environment are not high enough to be directly toxic to adult birds, but these pesticides accumulate in body fat and lead to the failure of some birds, notably the brown pelican and the peregrine, to reproduce. Dr Risebrough ascribed the reproductive failure of the brown pelican colony on Anacapa Island, off the Californian coast, in 1969-72 to excessive eggshell thinning caused by extensive DDT pollution from a Los Angeles factory, which in 1970 was discharging 500-1,000 lb of DDT daily into the sea.

Dr W. R. Bourne, from the University of Aberdeen, reported the distribution and movement surveys of seabirds around the coast of Britain during the past decade. Oil pollution was an especial hazard to the swimming birds, but gulls and waders were less affected. Oil damage seemed to be higher during January, February and March. Dr Bourne also reported that the population of auks, especially puffins, is decreasing, but the gull, fulmar and gannet populations are increasing. Discussing the proposed development of the oilfield off the north of Scotland, he thought that the greatest danger would come from disturbance of hitherto inaccessible habitats by road building and development rather than from large-scale oil pollution.

Dr N. W. Moore, of Monks Wood Experimental Station, pointed out that temperate and tropical regions present

different problems, as in warmer climates a greater use of pesticides is an economic necessity. In Britain, with its temperate climate, crops can be grown economically with the minimum use of insecticides, and the chief danger is from fragmentation of habitat because of the pressure on land. In North America, on the other hand, where agriculture has been concentrated into regions with intensive use of agricultural chemicals, pollution is a greater danger.

Dr J. J. M. Flegg, of the British Trust for Ornithology, called attention to the need for more research into the effects of natural factors such as climate, as well as the effects of urbanization and the loss of marshy and water habitat. Reproduction can be very variable, he said, and responses to changes are usually so delayed that by the time the effects have been recognized it is too late to apply any remedy.

Dr Duncan Poore, of the Nature Conservancy, said that serious conservationists face the dilemma that if positive proof of adverse effects is needed before action is taken, then it is usually too late to rectify the situation. So, asked Dr Poore, should action be taken on the facts in hand if an adverse effect is merely suspected?

ARC

Annual Report

Not surprisingly, in view of the publicity accorded the affairs of the Agricultural Research Council during the past year, the council's annual report for the year ended March 31, 1972, published last week (HMSO, £0.47), contains little that is new.

The year, according to the report, has been one of uncertainty under the threat of Lord Rothschild's axe and the report restates what the council emphasized during the debate on the government green paper on research and development, that priorities in applied research must "be assessed in consultation with the real customers (farmers, growers, food industrialists), scientists, economists and agricultural departments". The structure of the ARC, the report continues, was specially designed to plan and coordinate research, and offers "a ready made forum for the continuing dialogue urged by Lord Rothschild".

The government's decision on the ARC's future, made in July, will result in £5 million of the council's budget being transferred next April to the Ministry of Agriculture, Fisheries and Food, with a further £2.5 million being transferred on April 1, 1974, and a similar amount in 1975. These figures, based on 1971-72 prices, will remove more than half of the council's budget, but are less than the 78 per cent which

Lord Rothschild recommended should be transferred to the ministry.

In 1971-72 the council spent the full £19.96 million allocated to it by the Department of Education and Science, which is 16.6 per cent more than it received in 1970-71. But once inflation is allowed for, this increase becomes a more modest 5 per cent. The council also had receipts of £889,711, mostly from the sales of livestock and produce, and contributions from government departments and the Meat and Livestock Commissions. More than 50 per cent of the budget was spent in grants to independent research institutes—2 per cent more than in 1970-71—whereas research institutes and units of the council received a slightly smaller slice of the cake—their share dropped from 28.9 per cent to 28.3 per cent. Capital expenditure also decreased in 1971-72 from 14.7 per cent of the total grant compared with 16.4 per cent in the previous year.

MEDICINE

Developing Countries

MONEY spent on medical care in underdeveloped countries should be put to better use, according to an Office of Health Economics report published this week (*Medical Care in Developing Countries*, OHE, 1972). Statistics that show the numbers of doctors per head of population are deceiving, says the report, in that they do not take into account how the medical facilities are distributed between towns and rural areas.

The statistics also give no guide to how appropriate the services are to the community. The report claims that the building of prestige hospitals gives no benefit to the mass of the population and that a better way of spending the money would be to train medical auxiliaries to practise among the rural communities. The report says that the "barefoot doctors" of China are a suitable model for other countries to copy. It is estimated that the cost of training a doctor in a hospital in the Cameroons is £7,000, and a nurse £1,500, but a medical auxiliary can be trained for only £200.

The report also comes out in favour of health centres. The £2.3 million spent on one hospital could be used to build a hundred or so medical centres that would give better service to the 70 per cent of the population of the developing countries which lives in rural areas.

Medical care, however, is not the complete answer and the availability of pure water to a community is far more significant. Also the report says that it is important to educate the population in hygienic methods of sewage disposal in order to reduce infection.

NEW WORLD

Energy Crisis, Home and Abroad

by our Special Correspondent

THE great energy crisis promises powerfully to galvanize the new Congress, due to assemble for the first time in January, and several agencies of the Administration are busily preparing for the fray, but the reality of the phenomenon remains at least a little in doubt. Now that the summer has passed with fewer brown-outs than there might have been, dark hints are circulating that crisis talk may be a plot by oil and gas interests to win favour for price increases or by the nuclear power industry to win approval for the next round of nuclear power stations. And there is certainly a chance that the air of crisis is no more than a recognition that the time has come for the United States to be dependent on external supplies of petroleum and reconciled to the kinds of prices which are already current in Europe.

The ramifications of the problem have been dramatized by the trade agreement between the United States and the Soviet Union, signed in Washington on October 18, which among other things provides a framework within which the two countries may work out means for exploiting the natural gas in Siberia for the benefit of the United States fuel economy. The idea, apparently welcomed in Moscow, is that Western (but not necessarily American) capital would be used to finance the exploitation of the Siberian fields (with declared reserves of 565×10^{12} cubic feet of gas and estimated resources of six times as much) against long-term contracts with United States importers.

The sub-committee on Economic Policy of the House Foreign Affairs Committee has already heard several witnesses shake their heads over the prospect of economic dependence on the Soviet Union (which could nevertheless supply only about 5 per cent of the needs of the United States for natural gas in the early 1980s, when exploitation might begin). The Senate Interior and Insular Affairs Committee will take another bite at this hot potato before Christmas. Representative Wilbur Mills thinks there are risks, and the president of the National Coal Association has described the project, which private companies are now being asked to investigate in detail, as a "reckless gamble". Mr Peter M. Flanagan, President Nixon's advisor on international economic affairs, has asked

judiciously for a careful consideration of all the alternative external sources of natural gas, holding that national security is nevertheless compatible with a modest importation programme.

Gas imports will nevertheless raise unfamiliar spectres, as can be told from the speech by Mr Peter G. Peterson, the Secretary of Commerce, to the American Petroleum Institute on November 14. In Mr Peterson's view, the new development is that it is no longer possible to regard energy supplies as cheap. He said goodbye to the "happy era of low costs, low risks and high benefits" and with fine disdain for the Second Law of Thermodynamics complained that cheap fuel had provided no incentive for the electricity generation industry to increase efficiency above 40 per cent. In the new circumstances, however, all energy users would have an incentive to economy but, at the same time, "our international comparative advantage on energy is slipping away".

Mr Peterson explained that the Domestic Council Committee of the Cabinet has for several months been making a study of this "systems problem if ever there was one". His own view is that there will have to be a deliberate stimulation of domestic sources of energy. Talk of Siberian gas for the United States anticipated the solution of political, technical and financial problems not yet resolved, but Mr Peterson said that "foreign government sources and private sources have expressed interest in participating in major ways in financing the project" and that a multinational scheme would have political advantages.

Mr Peterson estimated that the cost to the United States of imported energy (mostly oil) in the early 1980s would be about \$20,000 million a year, and that Europe and Japan between them might be spending up to twice as much again. He therefore fears "a wild and cannibalistic scramble not only for energy but for external earnings" with which to pay the import bills, with "extremely rigorous competition" and export subsidies to boot. But the Soviet Union has no energy problem, which in turn suggests that a continuing energy imbalance in the United States "could have severe consequences for the entire free world". So might not the energy-importing nations find it advantageous to work out common policies?

Mr Peterson wagged a stern finger at the environmentalists, and asked

them to consider that the new regulations for automobile emission systems will increase fuel consumption by between 30 per cent and 40 per cent, partly at the refineries and partly in decreased efficiency. And if supertankers cannot be brought to the United States, transport costs for imported oil and gas will be increased by 6 per cent to half the landed cost. And the notion that it might be possible to "solve our energy problem by reducing our consumption" would have such painful economic consequences that the backlash might sweep away all the gains so far made in the "battle to protect the environment".

No doubt much of Mr Peterson's message will be reflected in a presidential message to Congress early in the New Year. And "you can expect his (the President's) decisions to be long ranged, tough minded, balanced, with full recognition of our role as citizens of the greatest domestic economy and the greatest economic and political power in the world".

Not even such a set of mosaic tablets will bring the several issues to an end, however. Even the future of electricity generation, for several years an apparently autonomous growth industry doubling every decade, is now in doubt at a year (1971) when output increased by only 5.4 per cent and when there was an increase (in real terms) in the price of electricity for the first time since 1946 (since when the delivered cost of electricity has fallen by a half). The average price of electricity, about 1.6 cents per kWh, increased by 1.8 per cent (in real terms) between 1970 and 1971, with the result that economists have begun to ask what will happen if, in fact, the price should double before the century is out.

Several forces conspire to encourage such an outcome, not the least of which is the continuing difficulty of knowing what will happen to the nuclear power programme. Much will turn on the hearings on the safety of nuclear reactors planned by the Joint Committee on Atomic Energy, now bereft of Representative Chet Hollifield, for long the kindest of all the Atomic Energy Commission's critics. Senator John Pastore, the new chairman, promises as thorough an investigation as the redoubtable radiation hearings of 1960, with as evidence-in-chief the report on reactor safety on which the AEC has been labouring for the past year.

CHILEAN ASTRONOMY

Springtime for Some European Astronomers

from a Correspondent in Chile

In the optical astronomy centre of the world that Chile has now become, the astronomers are reassembling after the various international conferences. The concentration of scientists from the Northern Hemisphere in this one country is remarkable until one remembers the unique viewing conditions that are created by the conjunction of the cold Humboldt current in the Pacific and the hot stable desert floor situated on the other side of the central range of mountains.

Weather Difficulties

Nevertheless, the astronomers are in fact muttering about the dreadful winter that is only just expiring, a winter which has produced grass in abundance as far north as La Serena and flowers on the desert slopes. There have even been two days of snow at La Silla, the observatory of the European Southern Observatory (ESO) situated at a height of 2,440 m some 30 miles inside the Atacama desert at south latitude $29^{\circ} 15' 26''$. The astronomers there, however, can now look forward to nine months or so of virtually unbroken clear skies. On the average, they can count on well over 2,000 hours of good seeing in a year and some 220 photometric nights. (A photometric night is defined as providing at least six continuous hours of cloudless sky.)

The most recent addition to the range of telescopes that ESO has assembled at La Silla is a 1 m Schmidt which is now completing its commissioning tests. Delays in the design phases and in component deliveries have held up the project somewhat, but soon the telescope will begin several years of star mapping. Although the aperture is a little smaller than that of the Palomar Schmidt (48 inches) the improved optics and seeing will mean that the photographs that are taken with it are at least as good. Moreover, so that the maps of the southern skies produced by ESO are exactly comparable with those of the northern skies prepared by Palomar, the focal length of the ESO Schmidt has been fixed to give photographs on an identical scale.

Quite apart from the intrinsic value of this sky survey, the telescope can be fitted with an objective prism for producing low dispersion spectrograms, so increasing its versatility and making it an invaluable precursor to the 3.6 m

telescope which should become operational in 1976.

On the same mountain peak at La Silla, ESO has four other telescopes in operation. First, there is a double astrograph of 40 cm and 25 cm aperture, currently concentrating on radial velocity measurements particularly in the Magellanic Clouds; second, a 1 m photometric telescope working on the measurement of star brightness when viewed through various standard filters—to this instrument a digital spectrum scanner under computer control is being added, which will increase its use by 40 per cent; third, a 1.5 m spectrographic telescope with a high dispersion spectrograph located at the four mirror Coudé focus and a low dispersion spectrograph with image tubes at the Cassegrain; and fourth, a 50 cm photometric telescope with computer controlled positional setting.

On the same site are two other telescopes, one of 50 cm belonging to Denmark, and a 60 cm telescope operated by Bochum University. A temporary addition, a 50 cm meridian instrument, will soon begin a $5 \mu\text{m}$ sky survey in the infrared.

Collaboration

The present facilities are such that, on average, ten astronomers and their technicians are kept continuously busy on the mountain. The observing programme of ESO is worked out so that some 30 per cent of the time available is allocated to ESO staff members (twelve experienced astronomers and four junior colleagues). These staff astronomers (who will normally have contracts with ESO for not more than two periods of two years) will probably spend two to four nights a month on the mountain. Apart from a little time in reserve, the rest of the observing time is earmarked for astronomers coming from the member states of ESO (Belgium, Denmark, France, German Federal Republic, Netherlands and Sweden). Individuals will be allocated up to a total of, say, twenty nights on any one trip, and will go to the mountain two or three times in the process. Pressure on the facilities is great, the demand being some three times the capacity.

The ESO also has a collaboration programme of research with the University of Chile in Santiago. As well as giving assistance with lecture courses

and seminars for Chilean students, the university operates at its Cerro Calán Observatory, east of the city, an ESO Danjon Astrolabe, which, in conjunction with a high quality timing laboratory, can make accurate (relative) measurements of right ascension. In the same observatory the Chileans have a collaboration programme with the Soviet Academy of Sciences, which has installed a big transit instrument for the absolute measurement of star positions.

North American Activity

Radio astronomy is not forgotten, and in a sheltered corner of the mountains at Maipú, near Santiago, the university is working in collaboration with the University of Florida on planetary research, particularly on Jupiter.

Some 30 km to the north-west of La Silla, and readily visible from it, is the CARSO observatory. Here the Hale Observatories, largely supported by the Carnegie Institute, have installed a 40 inch telescope and they expect to begin construction soon of a 100 inch instrument. The Canadians have, on the same site, a 24 inch telescope, and there is talk of a big joint North American venture.

About 100 km to the south of La Silla is the Cerro Tololo Observatory of the Association of Universities for Research in Astronomy (AURA), which is comparable in size with the ESO installation and boasts six telescopes in regular operation ranging from 40 to 150 cm in aperture. The showpiece of the observatory will be the 400 cm versatile instrument almost identical to the one that has been built at Kitt Peak in Arizona.

Some way to the north the Soviet Union has been prospecting several sites. The thinking seems to be centred on another very large project, possibly a twin of the 6 m instrument at Zelenchukskaya in the Caucasus mountains, but there is no formal proposition yet.

In its early days ESO did for a time contemplate a site in South Africa, but the site qualities led them, like the North Americans and it would seem the Russians, to settle in Chile. The international astronomical community that has been established there certainly benefits from the friendly and stimulating relations between the centres, and enjoys the continuous patronage of its host state in spite of the rather dramatic political changes that have recently taken place.

NEWS AND VIEWS

The Hunting of the S.N.U.

"It is reasonable to hope that in a not too distant future we shall be competent to understand so simple a thing as a star." When Eddington wrote these classic words in 1926, knowledge of much of the input physics required for the construction of stellar models was rudimentary or non-existent. Forty years later stellar theorists were confident that they knew essentially all the relevant details of the nuclear physics, the opacities, the equation of state and the composition of stellar material. The evolution of a star could be sketched from birth to death and only a few links in the model remained to be strengthened. This complacency has, however, been shattered in the past few years and, what is worse, on the Earth's own cosmic doorstep. The Sun is simply not understood.

The man largely responsible for the present discomfiture is Dr Ray Davis, junior, of the Brookhaven National Laboratory in the United States. For almost twenty years Davis, with a number of colleagues, has pursued a hunt for the elusive solar neutrinos. Deep in the bowels of the Earth (to avoid spurious background effects) 100,000 gallons of C_2Cl_4 act as a solar neutrino telescope. Neutrinos interact with ^{37}Cl to produce excited ^{37}Ar ; in periodic sweeps the ^{37}Ar can be collected, its decay observed, and hence a count made of the neutrinos that have been captured. The counting rate is conveniently expressed in terms of the s.n.u. (1 solar neutrino unit = 10^{-36} neutrino captures per sec per ^{37}Cl atom). Davis's result as of 1968 gave an upper limit of ~ 3 s.n.u. As background elimination has improved, the upper limit has decreased, until, in early 1972, the upper limit was reported to be ~ 1 s.n.u. Doubts have even been expressed that any solar neutrinos have been observed.

Why have these results caused such consternation? Neutrinos are produced as energetically wasteful by-products of nuclear reactions occurring near the centre of the Sun. Because their interaction with matter is so slight, they should provide an essentially unimpeded view of the solar centre. And as the relative importance of several competing reactions is chiefly a function of temperature, neutrino spectroscopy in theory provides a fine tool for probing the temperature of the Sun's centre, an extremely interesting quantity on which model pundits have pontificated, unchallenged, for years. The point is that, until now, a maker of solar models only had to arrange for his model to give out the present solar luminosity at an age of $\sim 4,600$ million years. With composition somewhat constrained by observation, a great deal of slack in the problem could still be conveniently buried in the central conditions. There was no constraint on the central temperature.

The first serious predictions in 1964 suggested a counting rate of ~ 28 s.n.u. Improvements in the input physics subsequently reduced this value. To the theorists' credit the process began before the 1968 observations. It has, however, apparently levelled off in recent years at ~ 6 to 8 s.n.u. In terms of the temperature of the centre,

the models suggest a value of ~ 15 million degrees, the neutrino "observations" at least 10 per cent less.

In 1968 a visitor to Hoyle's sadly defunct Institute of Theoretical Astronomy might have heard desultory conversation about one "last resort" method of getting out of the difficulties. Desperate measures, however, await desperate times, and it was not until this year that William A. Fowler expressed this possibility publicly (*Nature*, **238**, 10 and 24; 1972). The basic point is that the neutrinos provide a prompt view of the solar centre, a view that is only eight minutes old. By contrast, the photons are not seen until they have diffused, with inevitable smoothing, for $\sim 10^7$ years to the photosphere and from there to the Earth.

If the Sun is in a transient phase, the previously computed relationship between central temperature and surface luminosity need no longer hold. Earlier workers had tried mixing the solar interior continuously to keep the hydrogen content somewhat higher, and the temperature therefore somewhat lower, than in an unmixed evolved Sun, but without great success as measured by substantial reductions in the neutrino counting rate. Fowler suggested that the process may not have occurred continuously, and that there may in the recent past have been a sudden mixing, resulting in the required changes. No reason was given for the sudden mixing, other than that the radiative interior of the Sun is not all that far from convective instability.

Several people have taken up Fowler's challenge, among them Dilke and Gough (on page 262 of this issue of *Nature*). They have not only calculated the consequences of sudden mixing (which itself has a few surprises), but have even suggested a mechanism. They point out that normal steady evolution builds up a gradient in ^3He in the solar interior. An analysis of the growth of small perturbations suggests that the steep temperature dependence of the (^3He , ^3He) reaction may induce overstability of the infinitesimal g modes. Fresh ^3He may then burn so fiercely that it triggers finite amplitude convection. In the consequent changes, the neutrino production, after an initial outburst, becomes anomalously low, while the surface luminosity decreases for about a million years and then increases more slowly, to pass its present value after ~ 4 million years. In short, models consistent with the neutrino counts now seem possible. Dilke and Gough even suggest that the ice ages support their conclusions. It will be interesting to see whether terrestrial climatologists agree.

If this is indeed the explanation, stellar evolution is in for a period of agonizing reappraisal. The argument presented by Dilke and Gough can (and undoubtedly will) be attacked at several points. In an earlier controversy, Dilke remarked that "the Sun is not a cup of tea". "Quite so," replied Spiegel, "but then convection is not a piece of cake." It seems certain that many more will join the party before the leaves finally settle down.—J. F.

Consecutive Enzymes in the Glycolytic Pathway

EARLY crystallographic studies of globular proteins were carried out on proteins that could be obtained readily in large quantities, such as myoglobin, haemoglobin and extracellular enzymes like lysozyme and ribonuclease, and on them protein crystallographers developed an armoury of weapons with which they can now attack problems of increasing biochemical complexity. From being chiefly preoccupied with the tactics of structure determination, they can now afford to turn at least part of their attention to the strategy involved in learning as much as possible of the control processes of living systems. In place of the study of single enzymes in isolation, concerted attacks are now beginning in several laboratories on systems of enzymes such as those of the glycolytic pathway.

Enzymatic pathways are likely to have become more complex during evolution, additional enzymes having been introduced to give finer control, the specificity of each enzyme becoming narrower and the chemical change at each step becoming smaller; a structural similarity between neighbouring enzymes in a pathway might show that such evolution had taken place by gene duplication and divergence from a common ancestor. Efficient operation of the pathway might be promoted by direct interaction between enzymes, in which case it might be possible to see evidence of structural complementarity. More certainly, generally similar geometries might be expected in the substrate binding sites of neighbouring molecules, because the product of one enzyme is the substrate for the next. The subtle differences between binding sites, however, might be particularly informative in assessing the importance of distortion of the substrate conformation towards that of the transition state as a factor in rate enhancement by enzymatic catalysis.

The publication by Watson and his colleagues in this week's *Nature New Biology* (240, 130, 134 and 137; 1972) of preliminary studies of three consecutive enzymes from the glycolytic pathway represents another landmark in the development of protein structure analysis. While these papers contain some information of real importance, the crystallographer looks upon the successful calculation of a low-resolution map as having far more significance than the information that it directly reveals; it is an indication that the often frustrating search for pure protein, for suitable crystals for X-ray study and for the appropriate isomorphous heavy-atom derivatives have all been successful and that only a matter of time (and some effort) is required for a detailed picture to emerge.

The early stages of Watson's work on one of these enzymes, glyceraldehyde - 3 - phosphate dehydrogenase (GPDH), were by no means straightforward; the present work on the human enzyme was preceded by a lengthy and generally fruitless study of material from other species. Indeed even the human form has presented its special difficulties in the shape of a tendency for the crystals to twin, probably a consequence of the near but not exact 222 symmetry of the tetrameric molecule. Unfortunately, if this is the correct explanation, twinning may prove to be a recurrent problem in the study of oligomeric enzymes.

Watson, Duée and Mercer (*Nature New Biology*, 240, 130; 1972) have been able to establish the overall, nearly spherical, shape of the GPDH molecule and to identify,

at least tentatively, the positions of the boundaries between subunits. While crystal symmetry insists that the molecule has identical halves, the electron-density map shows that the two subunits in each half are also generally very similar. The similarity breaks down, however, in a localized region that lies close to the binding site of a heavy-atom compound known to inhibit the enzyme in solution. Watson *et al.* consider that it is premature to deduce too much from this observation, but it looks probable that there is an asymmetry of the subunits in their active site regions.

Comparison of the GPDH map with corresponding maps of lactate and malate dehydrogenases shows some possible similarities but also major structural differences. The true situation, together with the unequivocal location of the active site, is unlikely to be resolved until a more detailed map is available.

The structure analysis of yeast PGK, by Wendell, Bryant and Watson (*Nature New Biology*, 240, 134; 1972) has confirmed but added comparatively little to the picture of the PGK molecule from horse described earlier this year by Blake, Evans and Scopes (*Nature*, 235, 195; 1972). The molecule is a monomer of weight about 45,000 and in shape consists of two lobes connected by a narrow waist. Blake *et al.* found that Mg-ADP bound to one lobe, but the other seems to be too remote to form part of the same active site; they suggested that the second lobe might be important in control or in binding to another molecule, perhaps even another glycolytic enzyme. Wendell *et al.* suggest now that the PGK molecule might bend at its waist, allowing both lobes to form the active site in solution, and they hint at a slight difference in the relative disposition of the two lobes in the yeast and horse PGK crystals. Undoubtedly the eventual comparison of the two enzymes at higher resolution will throw some light on this question as well as showing the sort of variation undergone by the enzyme through a long span of evolutionary history.

The structure of the third enzyme of the series, yeast phosphoglycerate mutase, reported by Campbell, Hodgson and Watson (*Nature New Biology*, 240, 137; 1972), is again tetrameric with one exact two-fold axis relating identical halves, the two subunits in each half appearing to be very similar in conformation. The contacts between the two subunits in each half are more numerous than those between the two halves, suggesting that the molecule might dissociate into dimers fairly readily, and it is of interest that the corresponding enzyme from muscle is only dimeric. Preliminary attempts have been made to locate the active site of the yeast enzyme, which has been little studied chemically; in particular, no amino-acid sequence information is yet available, and Watson and his colleagues hope that their progress on the structure may stimulate activity in these directions.

These structural results have not so far revealed any grand design of the enzymes in the glycolytic pathway, the only family likeness discernible at this stage being a rather doubtful one between GPDH and other dehydrogenases. Nevertheless, the three structures have some features of real interest and one can look forward with some assurance to detailed models of a number of glycolytic enzymes becoming available within the next year or two.—A. C. T. N.

Internal Transmission of Dutch Elm Disease

It is not known whether the development of the aggressive type of elm disease (see Gibbs *et al.*, *Nature*, **236**, 121; 1972), which has spread so rapidly and seriously in southern England and Wales in recent years, has been caused by the introduction into Britain of a more virulent strain or strains of *Ceratocystis ulmi*, or whether such strains have arisen *de novo* within the population of the fungus already present.

C. ulmi is a highly variable species, both morphologically and physiologically, and its ability to generate new forms, including those of changed virulence, through sexual or other genetical mechanisms is well known. Alternatively, the early development of centres of epidemic infection in north Kent, south Essex, and near Gloucester, Bristol, Southampton and Ipswich, might suggest importation of the disease from abroad on timber, crating or dunnage. The introduction into and the development of the disease in these areas, either from an external or internal source, could well be associated with the relatively high incidence there of sawmilling and associated timber industries.

The account by Burdekin and Gibbs in this issue of *Nature* (page 306) describes for the first time good evidence in Britain (based on observations made on trees infected naturally and experimentally) of the direct transmission of infection to wood of the current year's annual ring from the annual ring produced in the previous year. Careful examination of the small trees involved suggested that the normal scolytid beetle vectors of elm disease were in no way associated with this internal spread of the fungus. Internal transmission was apparently confined entirely to aggressive strains of *C. ulmi*.

Burdekin and Gibbs describe the significance of these observations to the present policy of sanitation felling in which it is recommended that only those trees with more than half the crown seriously affected should be removed and the bark destroyed. If infection can spread from year to year within trees without the need for the introduction of new infection each year through beetle maturation-feeding wounds, the effectiveness of the present sanitation felling measures could be far less than previously envisaged. To a large extent, the appar-

ent small effect of the policy of sanitation felling on the spread of infection during the past two years has confirmed this fear.

The ability of aggressive infection to spread into wood of the annual ring produced in the following year may also be of significance in relation to behaviour of the bark beetle in seriously affected areas in which large beetle populations have been generated. Because of factors associated with population pressures and the physiological condition of trees with slight crown infection (or those in which internal water relations have been affected by other factors), breeding by beetles may be attempted in the bark of mainstems and large low branches in the late summer and autumn. Even though this activity is abortive, if the breeding tunnels reach the wood and limited infection of an aggressive strain of *C. ulmi* develops therein, development of infection from these foci into the large vessels produced in the spring of the following year could spread rapidly and have a much more serious effect on the tree than comparable infection introduced through maturation-feeding wounds on small shoots in the crown.

Although not mentioned by Burdekin and Gibbs, their observations

underline a further weakness in the current programme of control of elm disease, about which there has been considerable public comment recently. If infection (by aggressive but not non-aggressive strains) can spread from year to year within the tree, it would seem imperative to attempt to confine aggressive infection to within the perimeter of the expanding areas already affected.

At present the Elm Disease Order allows the transportation of unbarked or otherwise untreated logs from infected areas to saw mills, furniture factories, and anywhere in Britain, with lorries perhaps loaded with logs carrying large populations of beetles at all stages of development, passing through large areas as yet apparently unaffected by aggressive-type infection. While in passage on lorries, infective adult beetles may emerge, with particularly high risk of successful emergence occurring during stops in lay-bys and at roadside cafés. Relative to the possible environmental, aesthetic and financial consequences, the immediate financial benefit to the timber trade of free movement of logs must be very small indeed, and the continuation of this activity in the south of Britain would appear to be quite untenable.—R. G. P.

Normal and Abnormal Globin Synthesis

IN *Nature New Biology* next week (December 6) Gianni *et al.* describe the identification of a globin messenger RNA, not associated with polysomes, in the cytoplasm of rabbit reticulocytes. They observe that, unlike the messenger isolated from polysomes, it directs only the synthesis of α chains.

In a density gradient centrifugation two RNA fractions were found, one with a sedimentation rate similar to EDTA-released messenger ribonucleoprotein, the other lighter fraction in the haemoglobin region of the gradient. RNA isolated from the heavier fraction had a sedimentation rate of 10S when analysed on polyacrylamide gels, and the RNA of the other fraction was resolved into two peaks of 7S and 4S RNA. Melting profiles of the RNA from both fractions gave evidence of secondary structure and base composition studies indicated that the lighter fraction contained RNA rich in guanine. These characteristics suggested that the extra-ribosomal heavy fraction might be an α mRNA. The occurrence of excess α chain mRNA in the extra-ribosomal

fraction may tie up with the report by Lodish of a decreased rate of ribosome attachment to each α mRNA relative to β mRNA.

In the same issue of *Nature New Biology*, Clegg and Weatherall report that although they have observed the same discrepancy between the α/β globin chain ratios in the marrow and peripheral blood as previous workers, nevertheless there is marked chain imbalance in the bone marrow of β -thalassaemia. The apparent differences between α/β chain ratios in the bone marrow and peripheral blood can be explained, at least in part, by the increased rate of α chain degradation in the marrow, and by the presence of contaminating non-globin protein (especially in bone marrow) which co-chromatograph with β chains in the CM cellulose system used for globin chain separation. These results are not compatible with previous theories of unstable β chain synthesis in marrow; they suggest that there is a reduced production of β chains which may be attributable to a lack of β mRNA.

TRANSFER RNA

Search for Recognition

from our Molecular Biology Correspondent

THE sport of identifying the various functional sites on tRNA, by selective chemical modification, by studying activity in heterologous systems or, less fatiguingly, by looking for correlations between specificity and features of the numerous sequences now available for contemplation, has in general proved rather disappointing. In particular there is hardly any part of the structure that has not been implicated in the specific interaction with the aminoacyl tRNA synthetase.

A possible, though unpleasing, explanation is that the recognition mechanisms of different tRNAs for their particular synthetases have little in common. Workers in the field are not, however, as it seems, prepared to give up without a struggle, and attention is now being focused on one nucleotide in tRNA, the fourth from the 3' end. Roe and Dudock (*Biochem. Biophys. Res. Commun.*, **49**, 339; 1972) have homed in on this locus on the grounds of correlations that have emerged from heterologous charging experiments, in particular those in which a synthetase from one species aminoacylates the wrong tRNA from another. For instance, it has been known for some time that yeast phenylalanyl tRNA synthetase can operate on the phenylalanine tRNAs of various other species, as well as on a valine tRNA of *Escherichia coli*.

Roe and Dudock have now made a collection of nine *E. coli* tRNAs, all of which are charged by this same yeast synthetase. Aside from the nucleosides that are conserved in all tRNAs so far studied, the sequenced species that are aminoacylated by the synthetase, eight in number, all have twelve other nucleotides in common; eight of these nucleotides form the base-paired stem of the dihydrouridine loop of the clover leaf, one is in the loop itself, and another, an adenine, is the fourth residue from the 3' end. The other, as yet unsequenced, chargeable tRNAs will provide a test of the inferences, and in one of them—one of the *E. coli* alanine tRNAs—Roe and Dudock have already looked for and found adenine in the fourth position.

In other tRNA species the sequences are very similar, indeed in three (*E. coli* histidine, one of the *E. coli* glycine and yeast lysine tRNAs) all the critical residues for interaction with the yeast phenylalanine synthetase are there, except for the adenine at position 4, and these are not aminoacylated. By the same token, species which do have adenine in this position, but lack the requirements in the dihydrouridine stem, are equally ineffective as substrates. The requirements may, of

course, be narrowed down as new sequences come to light, but it seems entirely reasonable to infer that both regions of the molecule interact, at least with the synthetase in question.

Crothers, Seno and Söll (*Proc. US Nat. Acad. Sci.*, **69**, 3063; 1972) have simultaneously come up with a more explicit conjecture about the fourth nucleotide from the 3' end. Accepting with evident reluctance that no single part of the sequence can be regarded as the recognition site, and that the dihydrouridine stem sequence does not, as several lines of evidence on mutant species indicate, tell the whole story, they have designed their own universal recognition system. They allocate to one site a preliminary sorting function of the tRNAs into four groups, one for each base. This they refer to as a "discriminator site". After being found acceptable by this site, the tRNA will interact with the synthetase by way of one or more further sites.

But how to choose a criterion for the subdivision into four groups? The somewhat primitive choice of chemical (not steric) nature of the amino-acid seems to meet the requirement, when matched against the base in the fourth position from the 3' end. Actually Crothers *et al.* have been forced to add a fifth class, which slightly undermines the logic of their scheme, for whereas all other known tRNAs for any one amino-acid have a unique base in the critical position, the different arginine and lysine tRNAs from yeast have either a guanine or a uracil. For the rest, the correlation holds good, both for multiple tRNA of one amino-acid and between different species. Thus

for the non-polar side chains (alanine, leucine, isoleucine, valine and phenylalanine) the fourth base is always adenine; it is also adenine in tyrosine, methionine and cysteine tRNAs, and arginine and lysine tRNAs of *E. coli*. For serine and threonine, as well as both glutamic and aspartic acids and their amides, the fourth base is guanine; that for histidine and proline is cytosine, and that for glycine is uracil. The random probability of such a correlation, though difficult to compute in any rationally quantitative manner, seems to be small, sufficiently so as to persuade the authors that their pattern is not a mirage. They suggest that the fourth base could be involved directly in recognizing the amino-acid or, more likely, may be an evolutionary survival from a primitive, myopic, recognition system which could discriminate only between chemically similar classes of amino-acids.

Concerning a different part of the tRNA chain, the pseudouridine loop, an interesting observation has been made by Simsek and RajBhandary (*Biochem. Biophys. Res. Commun.*, **49**, 508; 1972). They have completed the sequence of the initiator methionine tRNA from yeast, and find that the universal —G—T— ψ —C— sequence is here replaced by —G—A—U—C—. The only other tRNA, in which the former sequence was reported not to occur, does not function in amino-acid incorporation. At another point in the same loop the initiator tRNA uniquely carries an adenine instead of a pyrimidine. The composition of the chain in this region presumably reflects the special function of the initiator.

New Assay for RD 114 Virus

THE intensive search that is currently being made for human RNA tumour viruses analogous to the RNA tumour viruses of lower mammals and birds has resulted in the isolation of RD 114 virus, a virus that has the biochemical and biophysical hallmarks of a typical C-type RNA virus but which has a unique intraspecies specific (group specific) antigenicity. This virus, which is released by human rhabdomyosarcoma cells after they had grown in the brain of a kitten, may yet prove to be the first human C-type virus to have been isolated, and what Rand and Long have to say in *Nature New Biology* next week (December 6) should prove of interest to virologists working with RD 114 virus or related viruses.

Rand and Long set themselves the task of devising an assay for RD 114 virus and have come up with a syncytium formation assay which is essentially similar to the so-called XC cell assay for mouse leukaemia viruses. Rand and Long overlaid confluent

monolayers of RD 114 cells liberating RD 114 virus with cells of a variety of human lines and then examined the cultures for syncytia. Cells of one line (KC cells), which are transformed by Rous sarcoma virus, formed abundant large syncytia either when they were co-cultivated with RD 114 cells or when they were exposed to purified stocks of RD 114 virus. By contrast, the untransformed parent cells from which KC cells were derived do not form syncytia in response to RD 114 virus or RD 114 cells.

Why and how KC cells respond to RD 114 virus by forming syncytia remains obscure; all that can be said at present is that the process depends upon intact RD 114 virus and treatment of the virus with either ether or detergents abolishes its ability to induce syncytia. But whatever the mechanism of the process proves to be the KC assay system provides not only an assay for RD 114 cells but also a method of screening for related viruses.

SATELLITE DNA

The Mystery Deepens

from our Cell Biology Correspondent

RECENTLY in *Nature New Biology* (240, 102; 1972) Mazrimas and Hatch reported a comparative analysis of the satellite DNA fractions in the genomes of several modern species of kangaroo rats and came up with the interesting speculation that this highly reiterated DNA might have some supragenetic function, for example facilitating recombination and, therefore, genetic diversity. Their basic observation is that highly specialized species of kangaroo rats, which occupy restricted ecological niches, have much less satellite DNA than species which have a broad geographical distribution and their speculation about the function of satellite DNA in recombination is at least as convincing as other suggestions that have been made to account for this portion of a genome, suggestions that include involvement in chromosome pairing or the maintenance of chromosome structure. But the fact remains that one is still at a loss as to the function of satellite DNA, the chief characteristic of which is its comparatively simple and highly reiterated base sequence, and indeed the more that is learnt about the distribution of satellite DNAs the deeper the enigma of its function becomes.

Recently in the *Journal of Molecular Biology* (71, 407 and 419; 1972), for example, Hennig reported the results of an analysis of the satellite DNAs in the chromosomes of *Drosophila hydei* which cannot easily be reconciled with the various functions that have tentatively been ascribed to satellite DNA. Apparently in *D. hydei* several fractions of satellite DNAs, detected by their rapid renaturation, have buoyant densities in the range of that of the bulk DNA and one of these "cryptic" satellite DNAs has a density identical to that of the peak of the bulk DNA. By breeding flies with different genetic and sex chromosome constitutions Hennig has proved that more than 60 per cent of the DNA in the heterochromatic arm of the *X* chromosome is satellite DNA whereas only 15 per cent of the DNA in the heterochromatic arm of the *Y* chromosome is satellite DNA. This finding should be remembered by anybody tempted to jump to the conclusion that heterochromatin can always be equated with satellite DNA. Hennig has also shown that the autosomal chromosomes of *D. hydei* contain 80 per cent of the total DNA of the genome but less than 10 per cent of this DNA is satellite.

Hennig's analysis of the distribution of satellite DNA in the polytene chromosomes of *D. hydei* fat body (each chromosome comprising 128 chroma-

tids) and salivary gland cells (each chromosome comprising 1,024 chromatids) has yielded even more interesting data. Apparently the proportion of satellite DNA in the *X* chromosome decreases with increasing polyteny. On the other hand, some repetitive DNA sequences in other chromosomes have a degree of polyteny as great as that of the non-repetitious sequences. Furthermore, after *D. hydei* DNA is partially digested with exonuclease III some of the fragmented DNA molecules reassociate to form circular structure. This implies, of course, that some highly repetitive DNA sequences are probably interspersed between non-repetitious sequences.

In short, when *D. hydei* chromosomes become polytenic some stretches of satellite DNA are under-replicated whereas others are replicated to the same extent as non-repetitious DNA. How differential replication of satellite DNA sequences is achieved and regulated, why such differential replication is advantageous and how the continuity of chromatids is maintained as this hap-

pens are questions that have to be added to those one has grown used to asking, about satellite DNAs in other organisms.

The origin of satellite DNA during evolution is another riddle, but Sutton and McCallum (*J. Mol. Biol.*, 71, 633; 1972) after comparing satellite DNAs in *Mus musculus* and asiatic species of mice from Thailand have recently proposed a possible mechanism. They have found that satellite DNA from several Thailand species is able to cross-reassociate with satellite DNA of *Mus musculus* to form poorly matched hybrid duplexes. From these and other data, notably analyses of pyrimidine tracts, they suggest some ancestral species possessed a short unstable sequence of perhaps only 8–20 base pairs. If, as different populations diverged, different mutations accumulated in this sequence and then, independently in the different populations, the diverging sequences were massively reiterated coincident with speciation, the result would be what is observed, namely a set of satellite DNAs with related but far from identical sequences.

Press-studs in Chromosomes

Engelhardt and Pusa report in next week's *Nature New Biology* (December 6) that use of the electron microscopy stain, ruthenium red, which preferentially stains for carbohydrates, leads to improved visualization of the elements involved in the attachment of chromosomes to the nuclear envelope. When stained with ruthenium red, nuclear pore complexes seem bipartite with the appearance of a "press-stud". 30–40 Å fibrils connecting condensed chromatin and telomeres with the inner half of the pore complexes are also clearly visible.

Nuclear pore complexes and the lateral elements of synaptonemal complexes of late pachytene are similar in terms of staining properties and structure. The lateral elements appear to be flanked on two sides by a row of ringlets facing the central space, which is probably filled with microfilaments. The authors suggest that the ringlets are the interior halves of the nuclear pore complexes which are deposited on microfilaments in the build-up of the synaptonemal complex, the microfilaments being derived from the inner nuclear membrane.

Another possible example of ringlets derived from nuclear pore complexes is found in the premeiotic *X* chromosome of *Acheta*. Ruthenium red staining reveals structures on the surface of the chromosome which appear to correspond to the inner halves of the bipartite nuclear pore complexes. Engelhardt and Pusa suggest, therefore, that the inner nucleoplasmic half of the pore complex is a permanent chromosomal element — "the cyclo-

mere"—which can be detached from the other half located in the outer nuclear membrane.

Suggested roles for these "press-stud" structures are manifold: synaptic detachment at zygotene, followed by restoration of chromatin-nuclear envelope connexions in pachytene may be implemented by pore complexes; the exterior halves of the complexes might peel off in nuclear envelope-derived annulate lamellae involved in the construction of the envelope at telophase; pairing of homologous chromosomes and/or intertelomeric or intercentromeric fibres may depend on the "press-stud" mechanism; the "press-stud" function in the nuclear envelope may be related to heterochromacy.

Cyclomeres may contain the proteins necessary for DNA replication. They might also be the sites of chromosomal acidic proteins, thus functioning in transcriptional control. There is substructural evidence, for example, that cyclomeres are transcription sites.

Cyclomeres may contribute to chromatin condensation, because hyaluronidase is an uncoiling agent. If cyclomeres characterize condensed chromatin then the bands of polytene chromosomes may turn out to be aggregates of cyclomeres. If these tentative roles are verified, then cyclomeres are alternatively bound to the nuclear envelope and free in different configurations depending on the functional phase they are in. Thus, as controlling and attaching elements, pore complexes and cyclomeres would play an essential part in chromosome organization.

ASBESTOS

Biological Effects

from a Correspondent

A WORKING group to review the biological effects of asbestos was held under the auspices of the International Agency for Research on Cancer at its new headquarters building in Lyon between October 2 and 5.

Asbestos is one of the most important air pollutants, and is widely used throughout the world for insulation, fireproofing, construction and other purposes. Inhalation of asbestos can lead to asbestosis, a fibrous reaction that impairs lung function, and to an increased risk of cancers arising within the lungs (bronchogenic carcinomas) or from the layer of cells lining the pleural and peritoneal cavities (mesotheliomas).

A great deal is known about the physics and chemistry of particles of the different asbestos fibre types that are widely used commercially: chrysotile, crocidolite, amosite and anthophyllite. The size and shape of the particles vary, and this affects the site of deposition in the lungs after inhalation. Thus crocidolite fibres from the North-west Cape Province of South Africa are straight and fine, and they penetrate into the terminal air passages more readily than the thicker crocidolite fibres from the Transvaal or the curly fibres of chrysotile. Studies with asbestos made radioactive in a nuclear reactor have enabled Dr A. Morgan (UKAEA, Harwell) and his colleagues to obtain useful information about the sites of deposition and fate of different fibre types.

The identification of different fibre types in small amounts, such as residues from lungs, has presented difficulties. Electron diffraction patterns of single fibres, however, allow ready distinction of chrysotile from amphiboles. Dr V. Timbrell (MRC Pneumoconiosis Unit, Penarth) has found that the amphiboles crocidolite and amosite, which are very similar in chemical composition and many other properties, can be distinguished by the preferred orientations which they exhibit in a magnetic field. Crocidolite is paramagnetic, so the fibres become aligned parallel to the field lines, whereas amosite, which is diamagnetic, aligns with fibres normal to the field lines. Using light scattering of aligned fibres in liquid suspension, quantities of the order of $1 \mu\text{g}$ can be resolved.

The biological effects of asbestos have been studied in cultures of macrophages and other cells, and in organ cultures of lung or pleura. Because of their length, asbestos fibres are often incompletely phagocytosed. Chrysotile is more haemolytic and cytotoxic than other asbestos types, and as pointed out by Dr A. C. Allison (Clinical Research Centre, Harrow), the cytotoxicity can

occur early, because of interaction with the plasma membrane, or later, as a result of disruption of the lysosomal membrane. The concentration of magnesium ions on the surface of the particles seems to play an important part in the interaction with the constituents of membranes, mainly glycoproteins. Dr K. T. Rajan (Penarth) reported that asbestos added to cultures of human pleura induces a proliferative reaction in mesothelial cells.

Epidemiological studies carried out in several countries have produced strong evidence of association between asbestosis and exposure to all types of asbestos, both in the production and manufacture of asbestos products and in their use for insulation or other purposes. There is no clear evidence of differences in the ability of various fibre types to produce asbestosis, but the disease occurs at a very high rate when there is particularly intensive exposure, as is the case for textile and insulation workers. The same relationships probably exist for carcinoma, with no important differences in risk from different fibre types, and again high rates in textile and insulation workers. The risk of carcinoma in asbestos workers is greatly increased by cigarette smoking, but whether the two effects are additive or multiplicative is not yet certain. For mesothelioma an even more extreme difference occurs between insulation and

textile workers as compared with others, and there is a strong suggestion that crocidolite is especially related to this type of tumour. There is, however, some evidence that chrysotile is not wholly free of risk in either production or utilization.

The effects of asbestos and other fibres on experimental animals, reviewed by Drs J. C. Wagner and G. Berry (Penarth) and Dr M. F. Stanton (Bethesda), correspond quite well with the epidemiological evidence of the effects of human exposure. Inhalation of the various fibre types results in pulmonary fibrogenesis and an increased incidence of bronchogenic tumours in animals. Intrapleural inoculation of all four principal types of asbestos fibre results in the induction of mesotheliomas in rats. Hence the low incidence of mesotheliomas in humans exposed to fibres other than crocidolite is probably a consequence of the relatively poor penetration of these fibres to the pleura and retention within the pleura.

Stanton reported that borosilicate glass and aluminium oxide fibres comparable in size to respirable asbestos, implanted in the pleura of rats, result in a high incidence of mesotheliomas, whereas larger or smaller fibres are less carcinogenic. These and other observations focus attention on the length and diameter of the particles rather than their chemical composition as important

Photoelectron Spectroscopy and Oxide Films

IN next Monday's *Nature Physical Science* (December 4) Hoar, Talermañ and Sherwood describe the application of a new technique, photoelectron spectroscopy, to the study of a long-standing but largely unresolved problem, the oxidation of iron surfaces. The technique, widely referred to as "ESCA" (a contraction of electron spectroscopy for chemical analysis, relies simply on the photoelectric effect, whereby electrons are emitted from the surfaces of solids (or from gases) when irradiated by photons of energy greater than the work function.

Modern instruments, such as that referred to in this study of iron, employ aluminium or magnesium X-rays as the excitation (the sharp $K\alpha$ emission line) and the photoelectrons are analysed electrostatically. This gives an energy spectrum of both core and valence levels for each element within approximately 10 nm of the surface. Such a "fingerprint" for each element, however, contains additional information concerning its chemical environment, inferred from the chemical shift (that is, the change in electronegativity on bonding) in the binding energy of the core levels. Hoar and his colleagues have used this facility to determine the charge state of the iron in oxide films on pure iron and an

iron-1.56 per cent aluminium alloy. The shift in the Fe-3p peak in fact indicates that the oxide on pure iron is Fe_2O_3 and that on the alloy is Fe_3O_4 . This observation clearly illustrates the usefulness of this technique in the field of corrosion, and indeed indicates its potential value in the study of any surface-chemical phenomenon.

Within this broader context, work is actively under way in many laboratories on such topics as heterogeneous catalysis, fracture and frictional phenomena, semiconductors and metals, and molecular chemistry (the latter in both solid and gaseous phases). The problems encountered vary from those of great practical importance, for example, catalysts for the control of motor car exhaust pollution, to those of esoteric theoretical significance in the study of the band structures of solids or bonding in coordination complexes.

Relatively few biological applications have so far been reported, although a group at the University of Nottingham plan to exploit the technique in the examination of teeth and their decay (essentially a surface phenomenon). There can be no doubt, however, that further applications in similar fields, such as forensic science, will follow shortly.

factors in carcinogenicity. Samples of asbestos extracted with organic solvents have produced as many tumours as the original samples, so it now seems unlikely that contaminating hydrocarbons or trace metals are required for the induction of mesothelioma or bronchogenic carcinoma by asbestos.

HYDROGEN BONDING

(HF)₂ Revealed

from a Correspondent

IMPORTANT progress has been made in the past year or so in understanding the simplest hydrogen bond in the gas phase, that in the hydrogen fluoride dimer (HF)₂. This may well prove to be the breakthrough which leads to a clarification of other cases.

It would be difficult to exaggerate the importance of hydrogen bonds. Without their special stability water would remain a gas down to low temperatures and almost all models in biophysics would be quite untenable. There is already a very large literature on the hydrogen bond, but real steps forward have proved elusive. This has been at least partly because intermolecular hydrogen bonding favours the liquid and solid states, whereas the gas phase is usually to be preferred when it comes to elucidating electronic and molecular structures—each molecule is then unfettered by interaction with its neighbours.

First there were the theoretical *ab initio* calculations of Kollman and Allen (*J. Chem. Phys.*, **52**, 5085; 1970) and of Del Bene and Pople (*J. Chem. Phys.*, **55**, 2296; 1971). Both groups agreed that in the equilibrium arrangement the two HF molecules remained as identifiable units with normal bond lengths. They agreed, too, that the hydrogen bond section F—H...F is almost linear, with a distance between fluorine atoms of about 270 pm (2.70 Å) and with the remaining hydrogen atom offset from this line. They disagree, however, about the exact size of the angle between the linear section and the second F—H bond. J. A. Pople in his recent Centenary Lecture to the Chemical Society confirmed that extended calculations support his earlier value of about 111°. The stabilization energy of the dimer is calculated as about 25 kJ mol⁻¹ and this agrees with such experimental evidence as there is. Surprisingly the hydrogen atom in the bond is slightly denuded of electrons compared with a hydrogen atom in a monomeric molecule of HF; correspondingly, the fluorine is slightly more negatively charged and is thus able to be on the receiving side of a further hydrogen bond so that stable chains and rings can be formed by aggregates of three or more molecules. For calculations

whose input is only the value of the atomic constants and the atomic number of fluorine the detail and reliability are impressive.

But most scientists place greater trust in experimental evidence and this has been provided even more recently. The difficulty with the (HF)₂ dimer is that it normally coexists with polymers of the form (HF)_n, where *n* is up to 6 or more; this reduces the effectiveness of many techniques such as electron diffraction. Dyke, Howard and Klemperer (*J. Chem. Phys.*, **56**, 2442; 1972), however, found that the hydrogen fluoride from an appropriate supersonic nozzle is cooled sufficiently for appreciable amounts of (HF)₂ to form, and they obtained satisfactory signals from their molecular beam electric resonance spectrometer using a simple mass spectrometer detection system.

The microwave transitions and the radiofrequency transitions of the Stark effect confirm the non-linear structure with an F...F distance of 279 pm and favour an angle of 108° between the HF bonds. Additionally they find an axial component of the electric dipole moment of 10.0 × 10⁻³⁰ m C (2.99 debye), and if the central hydrogen

atom is replaced by a deuterium atom the nuclear quadrupole coupling is 270 kHz. Although signals from F—D...FH were easily observed, no signals from F—H...FD were detected, suggesting that this isomer is considerably less stable. For the species (HF)₂ and (DF)₂ there was evidence for the exchange of the atom in the centre of the bond so that each hydrogen is there for half the time. The true description of the motion in this degree of freedom is analogous to the inversion mode in ammonia. It can be described by a "tunnel" frequency of about 19.776 GHz in (HF)₂ but only 1.580 GHz in (DF)₂ where the effective mass of the "tunnelling" hydrogen is so much larger; a crude estimate of the barrier height is 6 kJ mol⁻¹.

The mutual success of these two distinct approaches gives hope for their extension to H₂OHF and (H₂O)₂ and hence a thorough understanding of hydrogen bonds, at least in the gas phase. The work also confirms that both *ab initio* calculations, at a level which does not require excessive computer time and sophisticated molecular beam spectroscopy, are powerful tools for difficult structural problems.

Partons and the Origin of the Universe

GENERAL relativity explains the gravitational interaction in terms of four-moment of particles along geodesics in curved space-time.

An attractive programme is to explain away the other fundamental forces of physics in a similar fashion, and recent work by Wheeler and Misner has made a great deal of progress in this direction. The space-time in Wheeler and Misner's quantum theory is not only curved, but does not even have the familiar topology of Euclidean space. In Wheeler's world fundamental particles exist in worm-holes which are gaps in the space-time continuum.

The latest geometrical theory of fundamental forces seeks to incorporate strong interactions into the geometrical scheme. The present theory, due to Sarasatt, also incorporates the quark model of fundamental particles which provides a symmetrical explanation of the hierarchy of particles, although the prediction that quarks should be observable has not yet been fulfilled.

In an article in next Monday's *Nature Physical Science* (December 4) which refers to his work to be published in *Physical Review Letters*, Sarasatt considers the hypothesis that nucleons are made out of more fundamental units, whose structure is geometrical. These units are called partons. Their bare rest mass is enormous, 10⁻⁵ g, and their radius is 10⁻³³ cm. They carry the same spin as the electron and a sixth of the

charge. The large mass and small radius come from locating the singular surface which surrounds any massive particle according to general relativity at the worm-hole radius of a particle in Wheeler-Misner theory.

Partons bind together to form mesons and baryons because their gravitational attraction overcomes their electrical repulsion. This binding together takes place in two stages. In the first, three partons bind together to form a quark. Most of their resonance is lost in this stage in the form of emitted energy—photons, gravitational waves and neutrons. In the second stage, quarks bind together to form baryons and mesons according to the SU(3) scheme.

One attractive feature of the theory is that the huge energy release that takes place when partons coalesce might provide a source for the energy emitted from quasi-stellar objects.

It is known that solutions of the equations of classical relativity must contain singularities and such a singularity is the origin of the universe in the big bang model. Sarasatt conjectures that such singularities would split up the partons. This conjecture, like much else in this field, awaits the quantization of general relativity, but it provides a dramatic picture of the creation of the universe by the fusing together of partons to form nuclear matter, releasing energy which remains as the "black-body" background.

COSMIC RAYS

More about Muons

from a Correspondent

"THE possibility of a consistent explanation of various phenomena involving cosmic ray muons" is the intriguing title of a recent article by the Polish authors Wdowczyk and Zujewska (*J. Phys. A.*, **5**, 1514; 1972). As the title implies several recent cosmic ray observations have yielded results not readily explainable by accepted theories. Among these phenomena are the angular distribution of muon-poor air showers, the high energy muon spectrum derived from the burst size spectrum and the angular distribution of energetic muons observed by a group at the University of Utah.

The angular distribution of extensive air showers has been measured at large zenith angles ($>60^\circ$) by Catz *et al.* (*Proc. Twelfth Inter. Conf. on Cosmic Rays, Hobart*, 1030; 1972). They found many more showers than normal theories predict, and also that these showers were lacking in muons. The high energy muon flux at sea level derived from observation of the spectrum of burst sizes yields a flux higher than other methods, for example underground intensity measurements (see Chin *et al.*, *Proc. Eleventh Inter. Conf. on Cosmic Rays, Budapest*, 65; 1969).

Even stronger evidence that something was wrong seemed to come from the work of Keuffel *et al.* (*Proc. Eleventh Inter. Conf. on Cosmic Rays, Budapest*, 183; 1969). In the conventional picture of high energy interactions the dominant source of muons is the decay of pions and kaons. The competition between the decay and interaction of these parent particles should produce an enhancement of muon intensity at large zenith angles, but the Utah group have presented experimental evidence that above muon energies of 10^{12} eV this enhancement does not occur.

Wdowczyk and Zujewska attempt to explain these observations in terms of the presence of a partly passive, long lived, heavy particle (the X-particle) that is produced in the interactions of primary protons with air nuclei. Such a hypothesis has indeed been proposed previously to explain some of the effects. In order to bring some consistency into the explanations, however, the authors propose detailed properties for these X-particles. The production spectrum of X-particles above about 10^{12} eV is taken as parallel to the primary particle spectrum but with an intensity lower by a factor of 2,000. The lower limit to the cross-section for particle production is taken to be about 0.2 mb on the assumption that the X-particles are produced in pairs with the total energy of the pair half the primary energy.

Wdowczyk and Zujewska use the angular distribution of the muon-poor showers to give the attenuation length in the interval 1,000 to 2,500 GeV. The inelasticity coefficient is taken as 0.1 and the interaction length as 400 g cm^{-2} . The dominant decay mode is assumed to be $X^0 \rightarrow \mu^+ + \mu^-$ with a lifetime between 10^{-7} and 10^{-8} s, a limit set by the fact that the particle has not been observed in accelerator experiments but has to be consistent with the onset of the "Utah effect".

The model succeeds in predicting a sea-level muon energy spectrum consistent with experimental data. The muon spectra obtained from burst measurements are excluded, for on the basis of the model the bursts would be caused partly by interaction of muons and partly by the direct interactions of the X-particles. Wdowczyk and Zujewska also consider the burst size spectrum as measured, and show that this is in better agreement with expectations when the contribution from both muons and X-particles is taken into account.

The angular dependence of the in-

tensity of high energy muons is taken as a superposition of the angular variation of the intensity of muons produced by decaying pions and the variation of muons due to the X-process. By choosing the X-particle properties as detailed, an explanation of the Utah result is obtained. The authors also suggest that other cosmic ray phenomena, the presence of muon bundles in extensive air shows for example, can be explained on the basis of the same hypothesis.

It should, however, be pointed out that both the Utah effect and the presence of muon bundles have very recently been called into question by those who observed them. In neither case are the experimenters any longer prepared to stand resolutely by their previous results. The experimental backing for the X-particle hypothesis has therefore already been considerably weakened. Further experiments, based perhaps on the suggestions of Wdowczyk and Zujewska, have to be performed before confirmation (or denial) of the X-particle theory can be claimed.

Finite Elements for Alluvial Valleys

MATHEMATICAL understanding of the local effects of seismic waves is made difficult because of variations in structure and topography. Earth scientists are thus forced to oversimplify geological structures in any theoretical work, although numerical analysis is now yielding useful approaches to problems which theoreticians have hitherto regarded as insoluble. The finite element method, for example, has recently opened up new possibilities. This method was developed originally to solve stress-strain problems in aeronautical engineering but it has also proved useful in, for example, structural geology, dam engineering, rock mechanics and studies of tidal loading. In next Monday's *Nature Physical Science* (December 4), L. A. Drake describes an application of the method to the study of surface wave propagation.

Drake took an elastic finite element model of a valley in which a veneer of alluvium overlay a basin of shale and basement rocks. He used the model to look at the amplitude variation of a Rayleigh wave with a period of 2 s and found that the amplitude of the maximum ground motion was amplified by factors of about 270 and 35 in the horizontal and vertical components respectively. This result confirmed previous work by Mooney and Bolt (*Bull. Seism. Soc. Amer.*, **56**, 43; 1966), but the studies of these authors related to a plain layered structure only, whereas Drake was able to study the distribution of the ground motion across a geological

structure. He found that the maximum amplitude was offset from the centre of the valley. By varying the contact angle between the shale and basement no significant change was observed but the amplification of the horizontal component was sensitive to the lateral extent of the alluvial deposit. If the alluvium is deposited over a restricted area the amplification factor would be reduced.

Drake's use of the finite element method to look at the local effects of seismic wave propagation has many applications. The alluvial valleys of earthquake belts are frequently areas where the greatest damage to property and loss of life occurs in the event of a large earthquake. Even at considerable distances from the epicentre liquefaction, fracture, subsidence and landslips in unconsolidated material can occur; it should now be possible to predict more closely than before the effects of seismic disturbance.

The chief problems in applying Drake's technique to real structures are that geological models are difficult to obtain, that ground water may have significant effects and that the physical parameters of the rocks may be anisotropic and highly variable. It is, however, possible to place many of these factors into the finite element models: some will have little effect but others may be of great importance. Drake's models are a good first-order approximation and form a basis for further work which may help to reduce the risk of disaster in the seismically active areas of the world.

The Apollo 17 Landing Site

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Dr Lucchitta describes the geology of the Apollo 17 landing site in the Taurus-Littrow region of the Moon.

THE last lunar mission (Apollo 17) is to go to the southeastern rim of the Serenitatis basin, south of the crater Littrow and the Taurus Mountains. The landing site is on the flat floor of a narrow, graben-like valley trending east-west between steep, ancient mountain massifs (Fig. 1). Much of the valley floor and parts of the adjacent mountains are mantled by an anomalously dark and apparently thin and very young deposit. Some of the chief objectives of the Apollo 17 mission are to obtain samples from the very old and very young material which may be found in the massifs and the dark mantle, and to explore the valley floor underneath the dark blanket, exposed in crater walls and blocks near the landing site.

The massifs, located north, south, and southeast of the landing area, rise about 2,000 m above the valley floor, and have bright slopes of 25° to 30° with discontinuous ledges, rocky outcrops, boulder tracks, and abundant scarplets. Preservation of only young craters on the slopes of the massifs, and partial filling by debris of a crater at their base, suggest active mass-wasting processes. The massifs appear similar to the Apennine Mountains around the Imbrium basin near the Apollo 15 landing site¹. They probably consist of complex layered ejecta from old craters and the principal basins in the region; the thickest such layer is presumably derived from the Serenitatis basin, and a layer of Imbrium ejecta may be present

near the top of the sequence. Uplift of the massifs by faulting probably began with the Serenitatis impact event, but repeated uplift has maintained their steep slopes and the sharp topographic breaks at their base. An alternative interpretation of the massifs as young, viscous volcanic domes seems less likely because of the old craters present on their tops, and results from Apollo 15 and 16, which indicate a dominance of breccias in highland terrain.

Hilly material of lower relief than the massifs is widespread to the northeast of the landing area, and farther north near the crater Littrow. The hilly material also probably consists of crater and basin ejecta, including some from the Imbrium basin. The hilly morphology is thought to originate from disturbances related to basin-forming impacts, differential uplift along fractures, and mass-wasting triggered by seismic shaking. Lower hills, locally present around the edge of the valley and standing only slightly above it, are probably down-faulted parts of the adjacent mountains, possibly with products of mass-wasting and ballistic erosion from the surrounding units. All highland units discussed here are probably not significantly different lithologically, consisting principally of impact breccias; they differ chiefly in their apparent response to the deformational stresses that have affected the area.

Plains material underlies most of the dark mantle on the relatively planar, eastwardly inclined floor of the Littrow valley. A major scarp, several minor scarps, and numerous pits, grooves, and crater clusters are superposed on the unit. The evenness of the smooth plains suggests that the material was emplaced in a fluid state, perhaps as basin-derived breccias or as lavas, possibly mare basalts, which filled the Littrow trough and partially buried low hills there. This flooding occurred chiefly after the major episode of faulting of the massifs and before later minor structural adjustments. The plains material may have been emplaced as early as pre-Imbrian (post-Serenitatis) time, and locally as late as Copernican time. An Imbrian age is, however, most likely by analogy with plains material elsewhere on the Moon.

The dark surficial blanket mantles the valley floor with a deposit that may range from less than a few metres to tens of metres in thickness. It drapes over craters as young as middle Copernican, and seems to consist of overlapping patches of different shades of grey. Generally it seems that the darker the mantle the fresher or thicker it is. Within the highlands, very dark mantling occurs in streaks and patches on some steep slopes, and lighter mantling material is found in low areas. The material seems to be easily removed by mass wasting, for it is absent from most steep slopes and is concentrated in low areas. Large fragments are not visible in high resolution (2 m) orbital photographs, nor are they indicated by radar data². The dark mantle is most readily interpreted as a relatively unconsolidated pyroclastic deposit, most likely issuing from numerous small pits and fissures. It may have been deposited over a considerable period of time, but exposures in the landing area are of middle Copernican age and may locally be younger.

A bright mantle extends in a raylike tongue from the southern massif northeastwards across the valley floor and covers the principal part of the dark mantle, although locally the dark mantle seems to overlap it. The bright mantle appears to be relatively thin, and contains longitudinal grooves and ridges near its base at the southern massif. No blocks were observed, but radar data² indicate that it is coarser than the

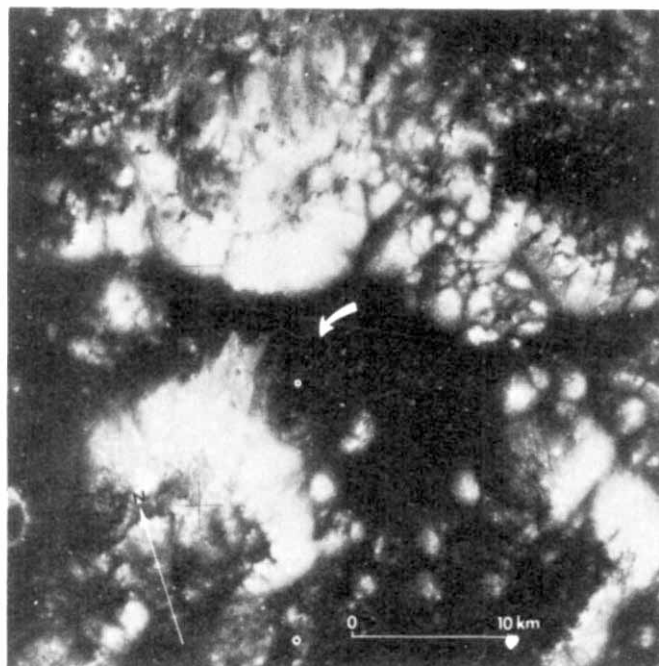


Fig. 1 Close-up of Apollo 17 landing area (part of metric photograph AS 15-0971). The projected landing site is indicated by the arrow. In the centre of the picture is a valley covered by a dark deposit and flanked by steep-sided massifs and hilly highlands.

dark mantle. It could be an avalanche of loose debris derived from the slopes of the massif and triggered by a cluster of small impacts or a violent seismic event.

Early and middle Copernican craters of up to 500 m in diameter are very common throughout the valley floor and most are covered by the dark mantle. Craters of middle Copernican age occur characteristically in northeast-trending clusters of partly irregular, overlapping craters, and are most readily explained as of secondary origin from a young, large and distant crater, possibly Tycho. Some, however, have faint dark haloes and may be volcanic vents. Craters of late Copernican age do not exceed 200 m in size, and have sharp rims and either very dark or very bright haloes. The craters with dark haloes may be volcanic or may be impact craters excavating and ejecting underlying dark mantle.

The area is transected by structures trending largely in northeasterly, north-northeasterly, and northwesterly directions. Much of the uplift of the massifs apparently occurred along these structural trends. Similarly, in the hilly areas, structural displacements follow lineament trends, and old crater rims are locally breached or partially offset by normal faults along these trends. Intense shattering of the lunar crust in this region may have resulted from the location of the area on or near the rims of several old basins (Tranquillitatis, Crisium, and

Serenitatis). A young structure extends as an east-facing scarp northwards across the valley floor and into the slopes of the northern massif without a break. Small splays and scarplets form *en echelon* patterns along some segments of this scarp and give the structure a mare-ridge-like appearance where it traverses the valley floor. The scarp may be a fault rather than an extrusive or intrusive feature. Relatively recent movement is indicated by the fact that it transects an Eratosthenian crater on the valley floor, and that it is preserved within steep mountain slopes. The scarp appears to be covered by the dark mantle, and therefore is older than the dark and bright mantles. An extremely sharp, subsidiary scarplet within the bright mantle may, however, indicate some movement along this structure after the bright mantle was formed.

This article is a summary based on studies of Apollo 15 panoramic camera photography for the Apollo 17 pre-mission geological map at a scale of 1 : 50,000 (ref. 3). I thank V. L. Freeman, D. H. Scott, and E. W. Wolfe for many helpful discussions.

¹ Carr, M. H., Howard, K. A., and El-Baz, Faroud, *US Geol. Survey Misc. Geol. Inv. Map I-723* (1971).

² Zisk, S., and Hagfors, T., in *Radar Atlas of the Moon*, NAS 9-7830 (Mass. Inst. Tech., Lincoln Lab., 1970).

³ Scott, D. H., Lucchitta, B. K., and Carr, M. H., *US Geol. Survey Misc. Geol. Inv. Map I-800* (1972).

A Man of Many Interests

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The work of Sir Grafton Elliot Smith was remembered at a special symposium held recently at the Zoological Society of London.

GRAFTON ELLIOT SMITH was born in Australia in 1871. After graduating in medicine at the University of Sydney he immediately began the researches on the comparative anatomy of the mammalian brain that were to make him famous. He had some interesting material to hand in the form of local marsupials and monotremes and he used it to unravel the homologies of the cerebral commissures (the fibre bundles connecting the two hemispheres) and of the intricate regions concerned mainly with the sense of smell. These were problems that had puzzled several very distinguished anatomists of a previous generation. After coming to England in 1896 he had the chance to gain an unrivalled experience of mammalian brains in cataloguing the collection at the Royal College of Surgeons. Here he examined specimens from a high proportion of the living genera of mammals as well as from many reptiles.

He soon perceived the importance of the corpus callosum, the commissure that joins those regions of the cerebral hemispheres which he named the neopallium. He chose this term because he realized that it was a new development in mammals, and he was able to show how the corpus callosum and neopallium evolved in concert. The transfer of the representation of vision, touch, hearing and motor control to the neopallium is a cardinal feature of mammalian organization, giving them powers of sensory discrimination and fine movement that surpass the reptilian level. He also studied the external form of the cerebellum and was able to devise a rational system for naming its parts, based on a consideration of its evolutionary development.

In dealing with the brain of placental mammals, Elliot Smith

devoted special attention to the Primates. The importance of vision in the life of this essentially aboreal group was a theme he never tired of expounding. He reasoned that it was the neopallial integration of information received from the eyes and from the sensitive, yet unspecialized, hands that gave the higher Primates superior appreciation of form and spatial relations. This was the essential prelude to the emergence of human tool-making skills and intellectual powers. These were imaginative inferences based on the location of functional areas on the surface of the neopallium and the supposed functions of the so-called "association areas" between them.

In mapping the functional areas Elliot Smith used simple methods, but his results were not very different from those achieved by others after laborious examination of nerve cell layers in the cortex. His diagram of the functional areas in Primates has often been reproduced. He also showed that the folds, or sulci, that are present on the surface of the hemisphere in larger mammals are often useful guides to the boundaries of functional areas.

It was a short step from his work on primate brains to his lasting interest in many facets of the evolution of man. When he went to Cairo as Professor of Anatomy in 1900 he had the opportunity to examine several hundred dissecting-room brains, and he became specially interested in variations in the form and position of the sulcus lunatus which bounds the visual area, the asymmetries of this part of the brain and its relation to speech and the use of the right or left hand. It was Elliot Smith, incidentally, who introduced the term striate area which is so often heard in physiological circles today.

The influence of his stay in Egypt did not stop at neurology, but drew him towards physical anthropology and the history of culture. He described skeletal material excavated in the Archaeological Survey of Nubia and wrote about the biological origins of the Egyptian people. His collaborator in this work was a fellow Australian, Frederick Wood Jones, who was also to write with imagination and perception on the evolution

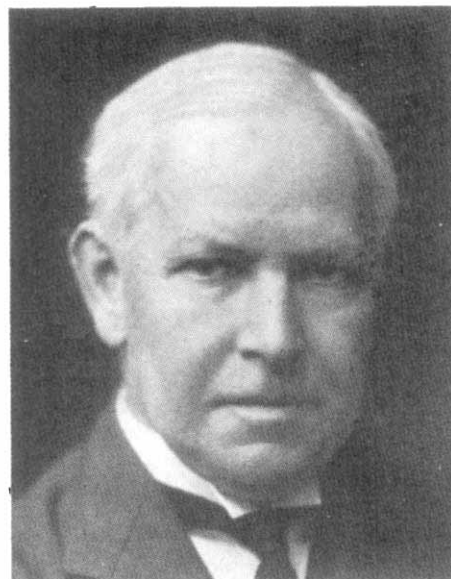
of Primates. In his capacity as an anatomist, Elliot Smith reported on many mummies, but soon he began to enquire into the methods of mummification, the social and psychological motives for this way of treating the dead and the reasons for its occurrence in widely separated parts of the world. It was the beginning of his excursions into sociocultural anthropology, which many have felt were his least successful ventures. He was an extreme advocate of the view that cultural inventions were seldom, if ever, made in more than one place and that widespread cultural resemblances must therefore be due solely to a process of diffusion. In many cases, so he believed, the diffusion centre was Egypt. As some of his critics protested, they had never doubted the importance of diffusion but simply denied that all cultural similarities could be explained in this way.

Elliot Smith's enthusiasm for the idea of culture diffusion led him to an interest in myths and in the symbolic significance of substances such as gold and blood and of various parts of the human body. He was influenced by the psychologically inclined anthropologist, W. H. R. Rivers, well known for his work in Melanesia on the Torres Strait Expedition, and he made an eloquent plea for psychology as the science that could unite the biological and social sciences. Unhappily this vision does not yet seem to have materialized.

In one of his later books, *Human History*¹, he declared that it is the thoughts and feelings that motivate human behaviour that are "the things that matter most". It is perhaps surprising that this utterance should have come from an anatomist, but it is a perfectly understandable outcome of Elliot Smith's long preoccupation with problems of the brain, an organ which, after all, is concerned with thinking and feeling. No doubt some social anthropologists, intent on keeping such untidy things as feelings out of their analyses of society, did not approve. Certainly many would not agree with him when he went on to denounce the search for laws governing human behaviour as a Cartesian error. He thought that the historical course of sociocultural change could sometimes be traced, but that accidents and quirks of individual personality played too large a part for it to conform to laws like the phenomena of physics and chemistry.

As an authority on the brain, Elliot Smith was often invited to examine the casts of the interior of the braincases of fossil men, and he was in close touch with the current work in human palaeontology. It was his student, Davidson Black, who boldly founded a new human genus, *Sinanthropus* (now *Homo erectus*) on the basis of a molar tooth from a cave near Peking. Later work amply confirmed the importance of this site, which yielded several complete braincases, many jaws and teeth and also stone artefacts. It was also a former student of his, Raymond Dart, who in 1925 described the skull of *Australopithecus africanus* from Taung in South Africa. Dart drew attention to certain resemblances to man and, by 1927, when the second edition of his *Essays on the Evolution of Man* appeared², Elliot Smith accepted this fossil as representing an early stage in the divergence of the human lineage, though it was relatively recent in date. He did not live to see the remarkable sequel to this discovery, the abundant new material found by Broom in South Africa and by the Leakeys in East Africa. Elliot Smith held the very reasonable view that the brain must have led the way in the evolutionary development of man. The African fossils do not conform to this idea, however, at least as far as the relative size of the brain is concerned. Their brains were small but the skeleton shows that they had gone a long way to achieving upright stance and locomotion. Some recent discoveries suggest, however, that forms with somewhat larger brains may have existed at the same time so that this conclusion is no longer quite so clear.

His conviction about the primary role of the brain also led Elliot Smith to welcome the Piltdown Man, a form which several authorities found disturbing and anomalous because it had quite a large and modern-looking braincase associated with a surprisingly ape-like jaw. He remarked on the co-



incidence that it was found within a month of his exposition of his views at the British Association in 1912. In retrospect it is sadly ironical to reflect that the persuasiveness of these views may have influenced the hand that perpetrated this monumental fraud.

Since Elliot Smith's death in 1937 human palaeontologists seem to have paid less attention to the somewhat blurred replicas of the cerebral surface that are provided by endocranial casts. They are perhaps too conscious of the formidable complexities of structure that lie below the surface to feel that much can be said about speech or other aspects of behaviour from such slight evidence. No doubt this critical restraint is beneficial, but one does sometimes feel that, after the daring sweep of Elliot Smith's vision, Primate comparative neurology has become a little parochial and dull.

Elliot Smith became Professor of Anatomy in Manchester in 1909 and moved to University College London in 1919. He must have had a prodigious capacity for work, for he published more than three hundred scientific articles as well as several books. In London, at the newly built Institute of Anatomy, his many-sided interests were represented, and he had rooms set aside for research in physical anthropology and ethnology as well as neurology. He was a great teacher and lecturer and it is interesting to read his refreshing views on the teaching of anatomy to medical students³. He believed that anatomists should not hesitate to use examples from zoology, physiology or pathology to give meaning to structure, and that they should try to instil a feeling for the living body. Lectures that merely repeated what was in the textbooks were a waste of time and the student should be encouraged to find out things for himself and not allowed to fall into the slough of rote learning.

It was characteristic of Elliot Smith that he was seldom content merely to describe, but was always seeking the wider implication of what he saw and striving for an understanding of evolution on a grand scale. He was never afraid to speculate boldly, feeling that it was "much more useful to make a concrete proposal that can be criticized and attacked than merely to play for safety". He had a reputation for enjoying controversy, even though he was a friendly and genial man, and he expressed his views with clarity and often with dramatic power. When he writes of the lower Primates as "laggards that turned aside from the highway which led to Man's estate" we seem to recapture some of the energy of his own forceful personality.

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¹ Elliot Smith, G., *Human History* (Jonathan Cape, London, 1930).

² Elliot Smith, G., *Essays on the Evolution of Man*, second ed. (Oxford University Press, 1927).

³ Elliot Smith, G., *Edin. Med. J.* (March 1918).

The Solar Spoon

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Overstability causes the Sun's core to mix every few hundred million years. This induces geological ice ages and temporarily depresses the solar neutrino flux.

SOLAR models are normally constructed by evolving for a time τ a stellar model of $1 M_{\odot}$ which initially had uniform chemical composition and was situated either somewhere on the Hayashi track or on the zero age main sequence. The principal direct observational data are the present luminosity L , effective temperature T_e , upper bound to the neutrino flux F_{ν} , and some knowledge of the chemical composition of the surface. The luminosity, effective temperature and chemical composition are essentially surface boundary conditions; the neutrino flux is an integral to which only regions very close to the centre contribute. Conditions can be imposed on the past history of the Sun by the theory of stellar evolution and by the requirement that our ideas about the history of the interstellar medium are not contradicted. But there remains considerable freedom in our choice of the initial abundances by weight, Y and Z , of helium and of heavier elements; further freedom is provided by our lack of knowledge of the precise value of τ , though limits can be provided by geological evidence and by measures of certain isotope ratios in the Sun.

There may be serious errors in the microscopic physics providing the equation of state, the opacity and the nuclear reaction rates, and our understanding of the macroscopic physics describing the fluid motions within the Sun is incomplete. In particular, the mixing length formalism normally used to describe the outer convection zone is unlikely to be accurate, but even if it were the mixing length l itself remains a free parameter (or, to be more precise, free function) of the theory. Varying l causes the relatively diffuse outer layers of the model to readjust so that the heat can still be transported. This has little effect on the structure of the central regions so L is hardly affected. Broadly speaking, therefore, l determines T_e , but in practice the converse is the case: the theoretical solar model serves to calibrate the mixing length theory for use in models of other stars.

Early measurements of F_{ν} (ref. 1) were considerably lower than solar models predicted at the time, and now it cannot be said with confidence that any neutrinos have been detected at all (Davis, R., jun., Wolfendale, A. W., and Young, E. C. M., in preparation). This has led to a re-examination of the microphysics and the general tendency has been to depress F_{ν} , although the latest predictions²⁻⁴ are still greater than the experimental upper bound. It is clearly worth re-examining the macrophysics too. In particular it is normally assumed that the solar core remains unmixed because the Schwarzschild criterion for convective stability^{5,6} is satisfied. Although Öpik has suggested that diffusion of material builds up a shell with high opacity around the core which eventually becomes convectively unstable and mixes⁷, his rough description of the diffusion process differs considerably from the results of more sophisti-

cated calculations⁸, and if one is to believe the latter it is unlikely that the mechanism, in unmodified form, can operate in a time shorter than the age of the Sun. The possibility that there may be a strong Eckman circulation^{9,10} has led to a consideration of the effect on F_{ν} of continuous mixing in the core¹¹⁻¹⁵. The results depend upon the timescale assumed for mixing and suggest that F_{ν} can be reduced, but it is not clear whether the reduction is adequate. In any case it seems unlikely that sufficiently rapid Eckman circulation exists in the Sun at the present time¹⁶. Here we consider the stability of the solar core and find a process which may lead to intermittent mixing; indeed Fowler¹⁷ has pointed out that such mixing may be even more efficient at reducing F_{ν} .

Instability of the Solar Core

In the core of a main sequence star the thermonuclear generation of energy has negligible effect on the Schwarzschild criterion, because the timescale for generating energy is very much greater than the growth time of convective modes, and so is always ignored when testing for convection. But energy generation might destabilize gravity modes in much the same way as Eddington¹⁸ thought pulsations might be driven in Cepheids. We consider a grossly oversimplified model: a plain-parallel stratified fluid layer of thickness d within which energy is being generated at a rate ϵ per unit mass. For the moment we assume that the only significant reactions are $p(p, \beta^+ \nu)D(p, \gamma)^3\text{He}$ and $^3\text{He}(^3\text{He}, 2p)^4\text{He}$ of the ppI chain which release energy at rates ϵ_{11} and ϵ_{33} respectively. Because it is unlikely that the solar core is in rapid rotation now¹⁶ we will ignore rotation altogether, and to simplify matters still further the layer will be presumed so thin that the Boussinesq approximation can be employed to describe buoyancy driven motions¹⁹ and gravity fluctuations will be ignored.

Because diffusion of material and momentum and the change of the H and He concentrations by nuclear burning are insignificant over the length and timescales considered in this section, the material derivatives of X and X_3 , the mass fractions of H and ^3He , will be taken to be zero and viscous stresses will be neglected. In cartesian coordinates (x, y, z) with z vertical, the linearized equations can then be written

$$\rho \frac{\partial \mathbf{u}}{\partial t} = -\nabla p' + \mathbf{g} \rho' \quad (1)$$

$$\text{div } \mathbf{u} = 0 \quad (2)$$

$$C_p \left(\frac{\partial T'}{\partial t} - \beta w \right) = \epsilon' + \frac{K}{\rho} \nabla^2 T' \quad (3)$$

$$\frac{\partial X'}{\partial t} + w \frac{dX}{dz} = 0 \quad \frac{\partial X'_3}{\partial t} + w \frac{dX_3}{dz} = 0 \quad (4)$$

where $\mathbf{u}$ is the fluid velocity and w its vertical component; p , ρ , T are pressure, density and temperature, K is the radiative conductivity $4acT^3/3\kappa\rho$, C_p is the specific heat at constant pressure, β is the superadiabatic temperature gradient and $\mathbf{g}$ is the gravitational acceleration. In these equations unprimed variables refer to the equilibrium state and primed variables to the perturbations. The equations must be supplemented by

an equation of state; the perfect gas law for a fully ionized gas, in Boussinesq approximation, will be assumed so that

$$\frac{\rho'}{\rho} = -\frac{T'}{T} - \lambda \frac{X'}{X} \quad (5)$$

where
$$\lambda = -\left(\frac{\partial \ln \rho}{\partial \ln X}\right)_{p,T} \approx \frac{5X}{5X+3}$$

With the help of this equation the perturbed energy generation rate is given by

$$\frac{\varepsilon'}{\varepsilon} = \eta \frac{T'}{T} + \nu \frac{X'}{X} + \nu_3 \frac{X'_3}{X_3} \quad (6)$$

where

$$\eta = \frac{\varepsilon_{11}}{\varepsilon} \left(\frac{\partial \ln \varepsilon_{11}}{\partial \ln T}\right)_{\rho, X, X_3} + \frac{\varepsilon_{33}}{\varepsilon} \left(\frac{\partial \ln \varepsilon_{33}}{\partial \ln T}\right)_{\rho, X, X_3} - \left(\frac{\partial \ln \varepsilon}{\partial \ln \rho}\right)_{T, X, X_3}$$

$$\nu = \frac{\varepsilon_{11}}{\varepsilon} \left(\frac{\partial \ln \varepsilon_{11}}{\partial \ln X}\right)_{\rho, T, X_3} - \lambda \left(\frac{\partial \ln \varepsilon}{\partial \ln \rho}\right)_{T, X, X_3}$$

and

$$\nu_3 = \frac{\varepsilon_{33}}{\varepsilon} \left(\frac{\partial \ln \varepsilon_{33}}{\partial \ln X_3}\right)_{\rho, T, X}$$

As is customary in initial studies of convective instability, the simplest boundary conditions will be assumed:

$$w=0, T'=0 \text{ at } z=0, d \quad (7)$$

After the usual manipulations²⁰ it can be shown that the space and time dependence of w , ρ' , T' , X' and X'_3 can be written in the separated form

$$e^{i\omega t} e^{i(k_x x + k_y y)} \sin\left(\frac{m\pi z}{d}\right)$$

where m is an integer. If $\omega^2 = \frac{k_x^2 + k_y^2}{k^2} \frac{|g|}{d}$ where $k^2 = k_x^2 + k_y^2 + \left(\frac{m\pi}{d}\right)^2$ equations (1) to (6) imply the dispersion relation

$$q^3 + E\alpha q^2 + \left[\lambda \frac{d}{H_x} - (\nabla - \nabla_{ad}) \frac{d}{H_p}\right] q + E \left[\left(\nu + \lambda\alpha\right) \frac{d}{H_x} + \nu_3 \frac{d}{H_3} \right] = 0 \quad (8)$$

where H_p , H_x and H_3 are the scale heights of p , X and X_3 in the equilibrium configuration, $\alpha = \frac{KTk^2}{\rho\varepsilon} - \eta$ and $E = \frac{\varepsilon}{\omega C_p T}$. The superadiabatic temperature gradient β has been written as $(\nabla - \nabla_{ad}) \frac{T}{H_p}$ where $\nabla = \frac{d \ln T}{d \ln p}$ and $\nabla_{ad} = \left(\frac{\partial \ln T}{\partial \ln p}\right)_{ad}$. Note that provided $k_x^2 + k_y^2 \ll k^2$, E is essentially the ratio of the free-fall time to the Kelvin-Helmholtz time for the core and is therefore small. Because on the main sequence the luminosity is largely balanced by the energy generated with the core, $\left(\frac{KT}{d}\right)^2 d^2 \approx \rho \varepsilon d^3$, where d represents the scale of the core, and the parameter $\frac{KTk^2}{\rho\varepsilon}$ is approximately $(kd)^2$; for the lower modes this is of order unity.

The dispersion relation (8) admits three roots. Granted that $E \ll 1$ the motion is nearly adiabatic and these roots may be written approximately:

$$q_{1,2} \approx \pm i \left[\lambda \frac{d}{H_x} - (\nabla - \nabla_{ad}) \frac{d}{H_p} \right]^{\frac{1}{2}} + \frac{1}{2} E \left[\alpha (\nabla - \nabla_{ad}) H_x + \left(\nu + \nu_3 \frac{H_x}{H_3} \right) H_p \right] / \left[\lambda H_p - (\nabla - \nabla_{ad}) H_x \right] \quad (9)$$

which correspond to the two g modes, and

$$q_3 \approx -E \left(\lambda \alpha + \nu + \nu_3 \frac{H_x}{H_3} \right) H_p / [\lambda H_p - (\nabla - \nabla_{ad}) H_x] \quad (10)$$

which corresponds to the secular mode. The f and p modes have been filtered out by the Boussinesq approximation. If

$$\nabla - \nabla_{ad} > \lambda \frac{H_p}{H_x} \quad (11)$$

the leading term of either q_1 or q_2 is real and positive, and provided this exceeds the small second term the fluid layer is unstable to direct convective motions. This criterion was obtained by L  doux²¹. But we pointed out above that solar models are stable to even the Schwarzschild criterion: $\nabla - \nabla_{ad} < 0$, and the g modes are oscillatory. This does not, however, imply that the core is stable; if

$$\left(\nu + \nu_3 \frac{H_x}{H_3} \right) H_p > \alpha (\nabla_{ad} - \nabla) H_x \quad (12)$$

the amplitude of the oscillations grows in a time comparable to the Kelvin-Helmholtz time for the core.

If the ^3He is in nuclear equilibrium with the hydrogen, $\frac{X_3}{3X} = \sqrt{\frac{\lambda_{pp}}{2\lambda_{33}}}$ where λ_{33} is defined in terms of the reaction rate r_{33} of the $^3\text{He}(^3\text{He}, 2p)^4\text{He}$ reaction by $r_{33} = \lambda_{33} \left(\frac{X_3}{3}\right)^2$ and λ_{pp} is defined similarly. $\frac{\lambda_{pp}}{\lambda_{33}}$ is a function of temperature alone, and $\frac{d \ln (\lambda_{pp}/\lambda_{33})}{d \ln T} \equiv -2b \approx -12$ at the centre of the Sun. Thus if the core is approximated by a polytrope of index n , $\frac{1}{H_3} = \frac{1}{H_x} + \frac{b}{(n+1)H_p}$ and the criterion (12) for overstability may be written

$$\frac{1}{H_x} > \frac{1}{\nu + \nu_3} \left[\alpha (\nabla_{ad} - \nabla) - \frac{b\nu_3}{n+1} \right] \frac{1}{H_p} \quad (13)$$

The analysis presented above very closely resembles the work of Defouw²². The thermodynamics of the overstability is identical, though the physical processes responsible for the energy source term ε are quite different. The existence of gradients in composition adds an extra term to the stability criterion: because in a hydrogen burning core $\nu > 0$, $\nu_3 > 0$, and X and X_3 do not decrease upwards, the perturbations in composition help to enhance the energy generation rate in a downward displaced fluid element relative to its surroundings and so add to the tendency towards overstability. Eddington's discussion of the overstability of spherically symmetric oscillations of Cepheids is similar, but in purely radial motion any displaced element always has the same fluid in its immediate environment and again the composition gradient does not directly enter into the stability criterion.

The existence of the composition gradients is also responsible for the secular mode whose growth rate is given by equation (10). Imagine, for example, a fluid element to experience a positive temperature fluctuation. It will rise to find a new hydrostatic equilibrium position which, because condition (11) is not satisfied, is stable on a convective timescale. Thus it will have the same pressure and density as its surroundings. But if $dX/dz > 0$, it will have a lower H abundance than its surroundings, and hence a higher temperature. Thus there will be a difference in the energy generation rate; if the increase resulting from the higher temperature, offset by thermal diffusion, exceeds the decrease resulting from the lower concentration of H and the inevitably lower concentration of ^3He , the temperature excess will be accentuated and the perturbation will grow.

An estimate of the growth rates of g modes in the Sun cannot be made by merely substituting typical values into

equation (9) because the perturbations will not be confined solely to the energy generating core. Outside the core the only surviving nonadiabatic effect is the radiative damping. Auré²³ has estimated that in early type stars damping in the envelope by far exceeds the excitation (in the semiconvective zone) beneath, primarily because the amplitudes of the perturbations increase rapidly with radius. But the comparatively deep outer convection zone in the Sun makes an important difference. When $\nabla - \nabla_{ad} > 0$ radiative heat transfer tends to destabilize the oscillation, just as it does p modes²⁴, and in any case the amplitude then decreases with radius. Further, one might expect the interaction with the unsteady direct convective motion in this zone to destroy whatever correlation the motion might have with the motion in the core. It is worth noting that if radiative damping is sufficient to make the effective value of α in equations (9) and (10) positive, the secular mode must be stable.

An approximate stability criterion for the Sun was derived by evaluating the work integral²⁵ for nonradial modes using the adiabatic eigenfunction for the fundamental g mode associated with the surface harmonic with $l=2$ published by Cowling²⁶ for the polytrope of index 3, and making the Boussinesq approximation in calculating the divergence of the radiative heat flux. The polytrope approximates the Sun reasonably well beneath the convection zone. In view of the more extensive analysis by, for example, Smeyers²⁷ of nonradial modes in other stars, one might expect the g_1 ($l=2$) oscillation to be a strong contender for the most unstable mode. Bearing in mind the discussion in the previous paragraph, the integrals were truncated at 70% of the total radius of the polytrope, the position of the base of the convection zone in the solar model considered in the next section. The analogue of criterion (13) for instability so obtained is

$$\frac{1}{H_x} > \left[(30 - \eta) (\nabla_{ad} - \nabla) - \frac{v_3 b}{n+1} \right] \frac{1}{(v + v_3) H_p} \quad (14)$$

where now one must enter typical values. In the solar core $v \simeq \frac{1}{2}$, $v_3 \simeq 1$, $\eta \simeq 9$, $b \simeq 6$ and $\nabla_{ad} - \nabla \simeq 0.1$, and the criterion becomes

$$H_x < 2.5 H_p \quad (15)$$

The Main Sequence Evolution of the Sun

The early main sequence Sun is roughly chemically homogeneous (H_x is infinite). The core is stable and no mixing occurs, except possibly at the very beginning when X_3 is far from its equilibrium value. A spatial variation in composition builds up until criterion (14) is satisfied and the core becomes unstable to oscillatory g modes.

What happens when large amplitude motions occur? If the core is mixed until the chemical composition and specific entropy are uniform, local nuclear equilibrium is upset: in the central regions X_3 increases proportionately more than X (because $H_3 < H_x$ before mixing) and the pp chain is dominated by the temperature sensitive ${}^3\text{He}$ burning reactions. The rate at which energy is generated close to the centre is increased by the redistribution of fuel; if, for example, an equilibrium abundance of ${}^3\text{He}$ in the inner 20% by mass is thoroughly mixed, the central abundance is increased by a factor of about 5 in a polytrope model and if the temperature were to stay constant the rate of the ${}^3\text{He} - {}^3\text{He}$ reaction would increase by a factor of 25. The consequent increased heat flux can no longer be stably transported by radiation alone, the Schwarzschild criterion for stability is violated and a mechanism to drive the motions originally postulated for mixing is thus provided. The core, though stable to infinitesimal direct convective modes, is unstable to finite amplitude convection.

The combination of overstability to infinitesimal disturbances and instability to finite amplitude convection has been studied in other contexts, for example, convection in a rotating layer of Boussinesq fluid²⁸ and thermosolutal convection²⁹. It is

thought that overstable oscillations first appear which ultimately grow until they are of sufficient amplitude to trigger convection. Laboratory experiments, though not conclusive, support this idea³⁰⁻³². The combination of the subsequent persistent convective motion and diffusion finally destroys or substantially weakens the stabilizing mechanism. We note that in the circumstances considered here, although diffusion of material was ignored when deriving the linear stability criterion, it was tacitly invoked later to render the solar core chemically homogeneous.

What are the evolutionary consequences for the Sun? As a result of transporting fuel to hotter regions of the star there is a tendency for the total energy generation rate to increase. But because the Sun is thermally stable this is adequately offset by an expansion of the core, converting thermal energy into gravitational energy, reducing the temperature and density and hence the energy generation rate. The timescale for this process, the Kelvin-Helmholtz time for the core, is shorter than the Kelvin-Helmholtz time for the whole star by a factor of about ten. So the envelope, which adjusts almost immediately to stay in hydrostatic equilibrium, at first expands almost adiabatically and cools. This causes a reduction in the effective temperature and surface luminosity. Meanwhile, as the ${}^3\text{He}$ abundance relaxes to equilibrium the temperature sensitivity of the energy generation is reduced and the convective core disappears. The bulk of the envelope has not yet had time to adjust thermally and the central temperature still remains anomalously low. Finally, in about 10^7 yr the whole star reaches thermal equilibrium; the central temperature, effective temperature and luminosity have increased somewhat, but not to their previous values, for the Sun is now chemically less inhomogeneous than it was before mixing began and takes on an appearance similar to when it was in a less evolved state. Normal main sequence evolution without mixing follows until condition (14) is satisfied once more, and the process is repeated.

To check this argument quantitatively, we modified a stellar evolution computer program provided by B. Paczyński. The only nuclear reactions considered were those of the pp chain, using published data for cross-sections¹⁵ and for the rate of electron capture by ${}^7\text{Be}$ (ref. 33).

The time which elapses between successive mixing phases is that required to build up a composition gradient sufficient to make the core overstable. This is a nuclear burning time and, taking criterion (14) literally, was found to be of the order of 10^9 yr. We emphasize that this value is not accurate because the coefficient in (14) was obtained by taking the difference between two numbers each of which was obtained from the structure of a particular g mode in a simple polytrope. We are now studying the stability of nonradial modes in a more realistic solar model. Because the Sun is now about 40% more luminous than it was when it arrived on the main sequence, evolution is occurring more rapidly. Consequently, provided the structure is not changing so much as to significantly alter the stability criterion, the period between successive mixing phases is decreasing. In the next section we argue that it is now about 2.5×10^8 yr.

To simulate the gross effects of earlier mixing a solar model was evolved for 4.45×10^9 yr with a core of mass m_c artificially mixed. Normal evolution was allowed for a further 2.5×10^8 yr, then the entropy and chemical composition of the core were instantaneously mixed and normal evolution was allowed to continue again. We found that a superadiabatic core did develop after the sudden mixing, though it did not extend to m_c . The velocity of the ensuing motion was sufficient to advect ${}^7\text{Be}$ away from the central region in a time somewhat shorter than the half life against electron capture. We assumed that mixing of ${}^3\text{He}$ and ${}^7\text{Be}$ occurs only in the superadiabatic region, because the amplitudes of at least the linear direct convective modes decay rapidly in the subadiabatic region surrounding the core³⁴, though finite amplitude motions may penetrate further³⁵. It can be seen in Fig. 1 that with $m_c =$

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BOOK REVIEW SUPPLEMENT

General Books

Ne Plus Ultra

Sir Peter Medawar

The Hope of Progress. By P. B. Medawar. Pp. 133. (Methuen: London, April 1972.) £2.

THE growth of our knowledge about nature has not made it any easier to reach rational decisions affecting man's fate. Instead, whereas the technological consequences of progress have rendered the making of such choices ever more pressing and their effects ever more grave, its intellectual consequences have made us aware of the difficulty, if not impossibility, of foreseeing the long-range results of our actions, while at the same time destroying the foundations for our judgment of their value. So what is to be done?

The collection of six essays, *The Hope of Progress*, by P. B. Medawar is a most important contribution to the effort to help *homo faber* escape from this dilemma. Medawar combines scientific excellence, erudition and literary skill with exceptional humanity and urbanity. I share nearly all the hopes and values expressed in these essays; so if in the following I take it upon myself to criticize, it is not because I suffer from the delusion that I can offer anything better, but because I think that the seriousness of the situation is highlighted by the fact that not even Medawar seems to be able to present an unambiguous philosophy.

One of the essays, "Lucky Jim", is a review of James Watson's *The Double Helix* and so not directly connected with

the main theme of the book. When Medawar wrote this review in the spring of 1968, he was nearly the only one (Robert Merton was another) among a host of reviewers who immediately grasped the literary and philosophic importance of Watson's book. Later that year, in my own review of the reviews of *The Double Helix*, I expressed the hope—now happily realized—"that Medawar's article will be reprinted in some anthology, so that it . . . can go on being read as an object lesson of what it takes to write a good review in the borderland between science and literature".

Three other essays—on the defence of the use of experimental animals to enlarge medical knowledge, the moral aspects and prospects for success of the deliberate manipulation of human genotype and phenotype, and the refutation of the grave charge that biology has now put it into our power to corrupt the body and mind of man—certainly bear on the hope of progress; but except for their literary style and erudition, they could have been written by any one of a dozen or so reasonable scientist-authors. I shall therefore focus here on "Science and Literature" and "On the Effecting of All Things Possible", which could have come only from Medawar.

The theme of "Science and Literature" is that these two important domains of creativity are not, as is often proclaimed, complementary and mutually sustaining endeavours to reach a common goal; instead, where they might be expected to cooperate, they compete for territory, and when literature arrives on the scene it expels science. Thus contrary to the more commonplace notion that when science arrives it expels literature, the shoe can also be on the other foot. According to Medawar, the mistaken idea of the complementarity of science and literature is a legacy of nineteenth century inductivist philosophers, such as Karl Pearson, and romantic poets, such as Matthew Arnold, who held that reason and imagination are antithetical, or at least alternative paths to truth, the former being the way of science and the latter the way of literature. But Medawar believes that imagination is part and parcel of doing science; science and literature differ in that

literary reasoning is a monologue of the imaginative voice unfettered by the nagging critical voice that asks whether what might be true is in fact the case. The supervision of scientific imagination by the critical voice demands that clarity is the chief criterion of good scientific exposition, whereas the freedom of the literary imagination from critical restraint gives voluptuary rhetoric free reign, often at the price of obscurity. Thus when the virus of uncritical imagination infects a territory of human inquiry to which science can lay claim, the scene becomes corrupt, a turf for mischief makers.

But are not men of letters, no less than men of science, after the truth? Yes, but according to Medawar, there is a difference between scientific and poetic notions of the truth: "When the word is used in a scientific context, *truth* means, of course, correspondence with reality." When used in a poetic context, however, truth may mean either "what *ought* to be", and thus be the revelation of an ideal, or "an alternative conception . . . which enriches our understanding of the actual by making us move and think and orientate ourselves in a domain wider than the actual", and thus be any "believable-in" statement. Since the poetic notion of truth does not ask for any empirical test, we need not be surprised that great difficulties arise when the life-style of the salon infiltrates the laboratory.

Medawar points to Freudian psychoanalysis as one of the glaring examples of the corruptive influence of the



Gunther Stent

literary syndrome. The psychoanalyst does bring some kind of order to incoherence, and, never being at a loss for an explanation, he may actually bring comfort or relief to his bewildered patient. He does so by building up a mythical structure which makes sense and is believable-in, regardless of whether or not it conforms to reality. But these procedures are "highly mischievous, not so much because they do harm or fail to do good, but because they represent a style of thought that will impede the growth of our understanding of mental illness".

This essay is followed by "A Reply to Sir Peter Medawar by John Holloway". After engaging in some literary one-upmanship, Holloway focuses on an important defect of "Science and Literature". For this essay fails to take account of why there is that difference between the notions of scientific and poetic truth. The reason is that scientific truth pertains only to that part of reality about which questions can be asked that admit clear yes-or-no answers. But poetic truths are meant to extend to those other, no less important, aspects of life which upon inquiry do not admit yes-or-no answers and about which one often cannot even speak plainly or unequivocally. Indeed, if art has any function other than entertainment, then it is to speak of the unspeakable.

Holloway's reply is followed by "A Rejoinder" in which Medawar does not deal with what, to me at least, appears to be the most important point raised by Holloway. I suspect that the unwillingness to admit, or even to consider, that there are aspects of reality which we are in desperate need of understanding but for which scientific truths cannot be established is a central, albeit unstated, premise of Medawar's philosophical outlook. It is difficult to be sure of this, however, since nowhere in these essays does Medawar explain his meaning of "reality". If reality is to take in only *events*, then the correspondence with reality of any statement about reality can, in principle at least, always be ascertained (for some statements in the microscopic domain even this bare-bones criterion fails to hold, of course). But if reality comprises also the causal connexions between events, then it is by no means sure that correspondence between reality and the products of imagination can always be established. This uncertainty holds particularly for events that occur in territories, such as psychology and sociology, to which, according to Medawar, both science and literature can lay claim.

Medawar's failure to concede the existence of this intrinsic limitation to our scientific quest for understanding the world is also part of the implied

infrastructure of his Rhadamanthine "Further Comments on Psychoanalysis" appended to "Science and Literature". I think he goes too far in relegating the whole of psychoanalysis to the ashcan. The trouble with therapeutic analysis is that since the conditions from which most patients nowadays seek relief have no objectively definable symptoms it follows that there does not exist any objective criterion of a cure. This was not so in the old days when Freud treated a hysterically blind patient; once that patient could see again, a cure had certainly been effected. Even if Medawar is right in his final judgment that psychoanalysis "is an endproduct . . . like a dinosaur or a Zeppelin; no better theory can ever be erected on its ruins", it may turn out that no better theory can be erected on any other basis.

In his introduction, Medawar mentions that in composing the essay "On the Effecting of All Things Possible" (which was his 1969 Presidential Address to the British Association) he took no particular pains to avoid giving "the impression of a moralizing discourse or sermon". The explicit purpose of this essay "is to draw certain parallels between the spiritual or philosophical condition of thoughtful people in the seventeenth century and in the contemporary world, and to ask why the great philosophical revival that brought comfort and a new kind of understanding to our predecessors has now apparently lost its power to reassure us and cheer us up".

Then literature had, as it has now, an inward-looking character and a deep concern with personal salvation and establishment of personal authenticity. Then people were, as they are now, oppressed by a sense of decay and deterioration. Then there was, as there is now, a feeling of despondency and incompleteness, a sense of doubt about the adequacy of man. Then intelligent and learned men sought, as they do now, comfort and mystical syntheses between science and religion, because of the rootlessness or ambivalence of their philosophical thinking. But these cries of despair were not then, nor are they now, necessarily authentic; they then tended, as they now again tend, to be an affectation of melancholy, a merely fashionable posture.

How did the English get out of their Jacobean rut? By having the first inklings of a new, hitherto unknown, cosmological concept: the idea of progress. This optimism arose from "the breathtaking thought that there was no apparent limit to human inventiveness and ingenuity. It was the notion of a perpetual *Plus Ultra*, that what was already known was only a tiny fraction of what remained to be discovered, so that there would always be more beyond".

So what is responsible for the present *Weltschmerz*? It is the rise of the belief that progress has been a bust: all that inventiveness, all that ingenuity, all those discoveries have brought us that monster, modern technology, which spawns new instruments of warfare and tramples down nature. How are we going to get out of our Space Age rut? By realizing, according to Medawar, that all this melancholy is quite un-called for, being based mostly on a superficial assessment of the human condition, and by hearkening, as did the Jacobians, to the Trumpeter of Hope, Francis Bacon, who praised "the virtue and dignity of scientific learning and of its power to make the world a better place to live in". I am not competent to judge the validity of the striking parallels which Medawar draws between the seventeenth and the twentieth centuries, but it is evident that however similar these two historical situations may be, they are sufficiently different that they could have totally different outcomes. If the lotus-eaters believe, as Medawar thinks they might, that it is time to stop because the growth of knowledge and know-how is reaching its intrinsic limits, they may not be convinced by Medawar's plea that there is no "intrinsic limitation upon our ability to answer questions that belong to the domain of natural knowledge and fall, therefore, within the agenda of scientific inquiry". For even if it were false, as Medawar says it is, to argue that the body of knowledge will soon be unmanageably large or complex, there would remain the deeper question whether the concept of scientific truth is applicable to all of nature. Medawar took the affirmative answer to this question implicitly for granted in "Science and Literature". But as Holloway's "Reply" indicated, there exists a fundamental abrogation of the "agenda of scientific inquiry", namely that in certain domains the best we can do is to attain poetic truths. I believe that much of the knowledge and know-how which we still lack has so far eluded our ken, not because we are still beginners with hope for improvement, but because it pertains to events whose causal connexions posited by the imagination are not susceptible to critical validation. Hence I allege that the limitless growth of knowledge hopefully envisaged by Medawar would occur in the salon and not in the laboratory.

The fate of the idea of progress in literature from then to now itself offers proof that there are limits to progress set not only by the impossibility in certain domains of inquiry of subjecting the products of our imagination to critical validation in the laboratory but also by apparent bounds on our very imagination in the salon. As Medawar points out, the seventeenth century was an

age of Utopias—ideal societies, whose perfection and desirability were believable-in for their contemporaries. And as in the eighteenth and nineteenth centuries the idea of progress became an article of faith—or with Darwin, a scientific fact—Utopias changed from eudaemonic wishing to optimistic prophecy. Utopias provide, therefore, important indices—not so much to progress itself—but to what people imagine progress over their actual condition would be. But by the middle of the twentieth century, the conception of Utopias came more or less to an end after Aldous Huxley had shown in *Brave New World* that what people had all along thought was progress actually leads to a horrible rather than wonderful human condition.

The reason why it is so difficult now to imagine a new Utopia is that the knowledge and insights we have acquired about the world since the seventeenth century have shown that the ensemble of human (all-too-human) aims and desires is self-contradictory and hence in principle unattainable. This was of little import at the long remove from the realization of any of these aims in the seventeenth century; but by the middle of the twentieth century the tremendous increase in human mastery of nature had finally brought these contradictions into the open. It is for just that reason that there has lately occurred what Medawar refers to as “a revival of faith in the Wisdom of the East”. For though this wisdom may be the nonsense that Medawar evidently finds it, it does, in its nonsensical way, try to resolve some of the contradictions by restructuring the ensemble of human aims. Hence I fear that even as eloquent a sermon as Sir Peter’s will not revive faith in the Wisdom of the West. What it would take is to put forward a believable-in Utopia to which progress could lead us in the twenty-first century. And that may be an impossible mission.

GUNTHER S. STENT

Biography of Appleton

Sir Edward Appleton, GBE, KCB, FRS. By Ronald Clark. Pp. xiii+240+24 plates. (Pergamon: Oxford and New York, January 1972.) £4; \$10.75.

MR CLARK’S informative account of Appleton’s life and achievements is welcome. He was an eminent discoverer, as his determination of the height of the ionosphere and many features of its structure and mechanism manifested. He had a passionate interest in scientific research for its own sake, and an equal devotion to “getting on”. He was born in Bradford, the son of poor, strict Wesleyan Methodists.

Virtually the only mark that separated his father, a warehouseman, from the working class was that he wore a bowler hat, not a cloth cap. Appleton went to the local elementary school, and won a scholarship to a secondary school. He got a first in the London Matric, and became a “temporary laboratory assistant” at Bradford Technical College. His old science teacher happened to meet him in the street, and advised him to compete for a local scholarship tenable at a university. Appleton won it, and went to Cambridge. He was a very typical Yorkshireman, keenly interested in cricket, with the stocky, muscular physique of the famous Yorkshire cricketer of the day, George Hirst, the greatest retriever of victory out of defeat in the game.

Appleton won prizes for mineralogy and physics, and in 1914 joined Lawrence Bragg as his first research student, beginning research on the crystal structure of copper. Very shortly after, the First World War started. After serving 116 days in the ranks, his application for a commission came through. He was sent for training for the Army Signal Service and attended the first radio course for officers at Malvern. This was the beginning of his concentration on radio; except for the war he would probably have become an X-ray crystallographer.

His first papers, published in 1918, were on thermionic valves. He returned to Cambridge in 1919, and when Rutherford became Cavendish Professor was appointed an assistant demonstrator. He started research on α -particles, but wanted to follow his own line, in which he was encouraged by Rutherford. He was stimulated by C. T. R. Wilson’s work on thunderstorms to correlate them with atmospheric. This brought him in contact with Watson-Watt; they collaborated for a time, but presently sharply diverged.

Research in 1924 led to the discovery that radio signals were arriving by two routes, one along the ground, and the other by reflexion from above. Hampered by a heavy load of teaching and being out of Cavendish atomic research, he applied for the Wheatstone chair at King’s College, London, and was appointed.

He succeeded C. T. R. Wilson as Jacksonian Professor in 1936, and when Rutherford died unexpectedly in 1937 he became Acting Director of the Cavendish. In 1938 he became Secretary of the DSIR. He concentrated on holding it together during the Second World War, so that it might assist industry to recover more quickly when the war was over. Though he was on military scientific committees his outlook was civilian. He kept his personal research on the ionosphere going, and

he could be found in his DSIR office correcting the typescript of his latest ionosphere paper, while the V-1 bombs were exploding overhead.

After he became Principal of Edinburgh University in 1949, and engaged in the immense task of extending it, he still sustained his radio researches, to the end of his life.

J. G. CROWTHER

There was Corn in Egypt

Who Will Eat?—The World Food Problem: Can We Solve It? By Michael Allaby. Pp. 252. (Tom Stacy: London, September 1972.) £2.90.

R. A. McCANCE, in a review of the literature of hunger oedema written as a preface to an experimental investigation of the state of nutrition in 1946 in Wuppertal in the Ruhr, listed sixteen closely printed pages of references to earlier occasions when hardship, malnutrition and famine had previously been reported in various parts of the world. These ranged through recorded history from Hesiod’s time in 700 BC, that of Diogenes Laertius in AD 300, from *The Chronicle of Novgorod* in the eleventh century to J. M. Good’s *Dissertation on the Diseases of Prisons and Poor-houses* of 1795 and so on to Ancel Keys in 1946. Famine, starvation and hunger have, alas, been part of the human condition since the beginning of recorded time. McCance and his colleagues could hope, by using the tools of science and the knowledge of their predecessors, to discover something of the physiological mechanisms by which lack of food causes the damage it does. There is less evidence to support the belief that polemical writers, focusing their attention on the here and now as seen by other reporters of the current scene, can draw up a plan by which all the inhabitants of the world can uniformly be assured of a nutritionally satisfactory diet. Even in biblical times, as we are told in Genesis, chapter 41, verse 57, “all countries came into Egypt to Joseph” (that excellent, if pre-scientific, administrator) “for to buy corn: because that the famine was so sore in all lands”.

No one can quarrel with Michael Allaby’s conclusion either that the human population cannot grow in numbers indefinitely or that the remarkable increases in food supplies brought about by agricultural science must some day reach their limits and stop, although it hardly reinforces his argument relating underfeeding with overcrowding to cite Great Britain as an example of an overpopulated territory. And it is even less easy to be convinced by his thesis that because after nearly half a century of the successful use of one kind of sheep dip or another kind of plant pesti-

cide their efficiency begins to diminish, the people who ate the extra food gained from their use did not benefit therefrom or that the skills of our successors may not regain the initiative. Mr Allaby has been diligent in collating information and opinion from a wide range of sources. When his authorities disagree, however, it is not clear what prompts him to favour one rather than another. When there appears to be a difference of opinion between, for example, the Agricultural Advisory Council of the Ministry of Agriculture, a speaker at the Farmers Club and the Soil Association about factors affecting soil structure relating to its food-producing potential, he appears to support most wholeheartedly the views of the last rather than the first.

Mr Allaby would like this to be a better world than it is. The rich nations, he thinks, must help the poor, but not indiscriminately. "The attachment of strings to aid may be the only quick way of bringing about vital reforms," he writes. By pulling these strings, "governments and forms of government must be replaced (and) existing social structures, disrupted already by over-exposure to western values and ideas, must be overturned". It's all so different from Joseph's prescription. He merely "opened all the storehouses and sold unto the Egyptians" and also—after a certain amount of argument, to be sure, to his ten brothers when they came on a purchasing mission from the land of Canaan.

MAGNUS PYKE

Epistemology and Science

Human Understanding. By Stephen Toulmin. Vol. 1: General Introduction and Part 1. Pp. xii+520. (Clarendon: Oxford; Oxford University: London, May 1972.) £4.75.

In this, the first of three volumes, Toulmin confronts the frustrating isolation of the methods of historians and philosophers of science from the procedures of scientists themselves, seeing this as unfruitful and unnecessary. He asserts that the isolation, and consequent misunderstandings, have arisen from a confusion between logic and reason in philosophies of science. Toulmin's self-appointed task is to reform our ideas about rationality and human understanding, so as to bring them into a closer relation with the actual procedures of scientists and other rationally acting professionals, for example, lawyers. He does this in a way that may be disturbing to many academic philosophers, but deserves very careful attention from scientists, philosophers and historians alike; for if what he has to say is, as I believe, in any sense valid, then it will help scientists

towards a more explicit understanding of their procedures in both historical and philosophical contexts. The intellectual cross-fertilization that is invited cannot but be beneficial.

The philosophical discussion may seem abstruse and remote to the outsider, but the difficulties are worth surmounting. To accomplish his aim of restoring the links between epistemology and science, Toulmin has to reject what he calls the seventeenth-century epistemic picture "of Man the Rational Knower facing Nature the Unchanging Object of our Knowledge" (a picture perhaps already outmoded among theoretical physicists at least), in favour of a more Heraclitean picture, and concurrently to develop a new theory of rational conceptual change.

The result is a profoundly thoughtful volume, full of destructive and constructive insights, clearly, cogently and thoroughly, if at times heavily, expressed. It should stimulate controversy, and prove especially fruitful historiographically, for it uses the history of science, generally skilfully, not to illustrate a philosophical thesis, but rather to construct one. The construct reflects back on its materials in a way that reinforces an evolutionary interpretation of historical change, where "the key notions are 'adaptation' and 'demand', rather than 'form' and 'validity'". In the process of establishing this picture that, with the necessary qualifications Toulmin imposes, works very plausibly, there are casualties. In an incisive section, Kuhn's explanations of scientific change are ruthlessly and convincingly demolished, with arguments logical and historical, the latter largely of his own making.

An evolutionary interpretation of historical change involves a populational analysis of concepts, and Toulmin considers the goals, problems and historical development of intellectual disciplines, as well as the organization and evolution of intellectual professions, with the matrix of historical, social and intellectual factors that render novel concepts authoritative. The social factors especially militate against a merely logical interpretation, so that the negative aspects of Toulmin's thesis are steadily strengthened.

The final section of the book considers the variety of rational enterprises, from non-scientific disciplines (raising interesting questions about the relations between science and technology) through to nondisciplinary activities that are none the less rational. Legal procedures, among others, are appealed to, to show how rational cross-comparisons may be made within and between disciplines.

Toulmin gives explicit intellectual support to some promising but hitherto ill-formulated trends in the historio-

graphy of science (including a fusion of the synchronic and diachronic aspects of the subject), offers grounds for methodological change in the social sciences, and runs directly counter to a good deal of philosophy, perhaps "psilosophy", of science. His wish to produce a more healthily self-integrated epistemic structure is one that commands sympathy, while his radical proposals to cure a deep and divisive sickness seem the most promising in recent years. The next two volumes (*The Individual Grasp and Development of Concepts*, and *The Rational Adequacy and Appraisal of Concepts*) are eagerly awaited.

TREVOR H. LEVERE

Technology and Ecology

Ecology, Pollution, Environment. By Amos Turk, Jonathan Turk and Janet T. Wittes. Pp. ix+217. (W. B. Saunders: London and Toronto, 1972.) £1.70.

The Careless Technology: Ecology and International Development. Edited by M. T. Farvar and J. P. Milton. (The Record of the Conference on the Ecological Aspects of International Development convened by the Conservation Foundation and the Centre for the Biology of Natural Systems, Washington University, December 1968, Airlie House, Warrenton, Virginia.) Pp. xxix+1030. (The Natural History: Garden City, New York, 1972.) \$25.

Pollute and Be Damned. By Arthur Bourne. Pp. viii+216+14 plates. (J. M. Dent: London, 1972.) £2.95.

THESE three books are entirely different in scope and treatment, yet they nicely complement each other. *Ecology, Pollution, Environment* examines the main types of pollution—agricultural, radioactive, thermal, noise and so on, and the parts of the environmental system they affect—air, water, land and people. In each case the necessary scientific basis is explained in simple terms, making good use of everyday examples. The whole treatment is ecologically based (although the senior author is a chemist), and the exposition of ecological principles is clear and well-illustrated. In particular the treatment of human population is lucid and deals with some of the commoner fallacies which attach to this subject. This book could form the basis of upper school and first-year university courses, and should be understandable even for students nurtured in the humanities.

Ecology, Pollution, Environment, by its very nature, is contributing nothing factually new. In contrast, *The Careless Technology* consists of fifty papers, the great majority being specific case studies. The title of the book is misleading: it is not an indictment of the technological extravagances of the

industrialized nations, but is, in fact, the published papers of a conference held in December 1968 at which the ecological effects of development projects in underdeveloped countries were examined. More than sixty scientists with direct experience in the field attended, and one of the most interesting parts of the book is the verbatim discussion which follows each section. The papers are grouped under such headings as irrigation (including dams); health and nutrition; consequences of drives for increased productivity (especially the effects of insect pest control); and environmental degradation by other practices. It is often depressing reading; paper after paper concludes with a statement such as: "Because development activities have not been coordinated within a comprehensive ecologic framework, there has been a drastic deterioration in the social and economic conditions of life."

How is it that elaborate and often expensive schemes can be put into operation and then found to be so disastrous ecologically? The members of this conference in 1968 had to ask whether ecological factors prevent the development of the underdeveloped countries in the direction of increased technological advance such as the developed countries have followed. The implications of such a question for the developing countries might be unacceptable if it were not that serious doubts are growing about the environmental stability of the technologically developed countries themselves. In

terms of ultimate survival those countries not irrevocably wedded to the philosophy of the developed countries may yet prove to be the less handicapped. In *Pollute and Be Damned* Bourne looks at the environmental damage which is going on in the world, not least as the result of technological development, and asks questions about the basic motives of man and his ingrained disregard of things environmental. Whereas *Ecology, Pollution, Environment* was an elementary textbook, *The Careless Technology* a collection of research papers and case studies, *Pollute and Be Damned* takes the broad view which is essential as a stocktaking, and draws some disquieting conclusions. Man has not yet come to terms with his environment, and, even more basically, with himself as an organism dependent on its environment.

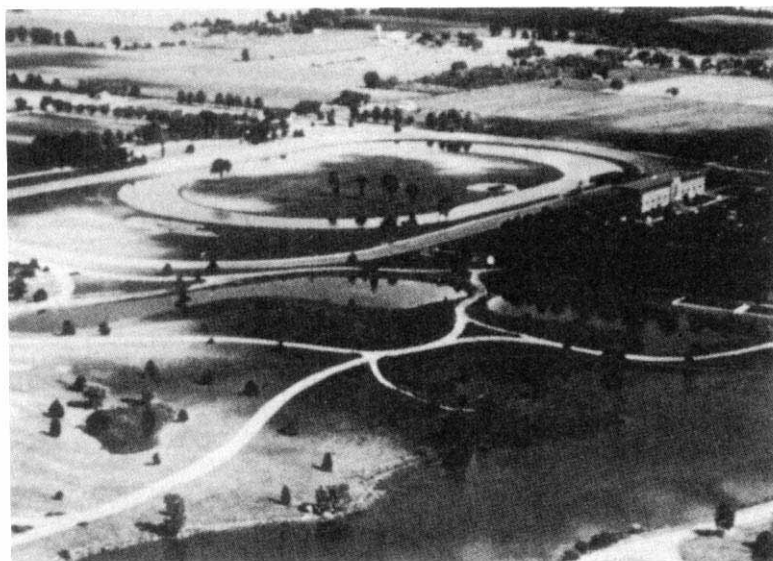
Bourne, a marine biologist, in places overstates his case and is at times in error. Lake Erie is not "completely dead to aquatic life", and not "all rivers ultimately empty their burdens into the sea". In fact, land-locked hydrologic systems constitute some very serious conservation problems, as the paper by Böckh in *The Careless Technology* shows. Nevertheless, a very consistent picture emerges from Bourne's book. He has had a good deal of contact with the official mind and one can see this so often at the root of the gross errors recorded not only by him but also by many of the contributors to *The Careless Technology*.

Had *The Careless Technology* not

taken nearly four years in gestation, some recent outpourings, suggesting that man only has to bend his mind to his present predicament to put things straight, might have been stifled at birth by the repeated examples of failure which it contains. As Bourne puts it: "The genius of modern man is for making mistakes." In all this there is no very obvious sign of that highly successful organism, technological superman. Nevertheless, there is some evidence that we are gradually turning the ship round. In 1956 there was published another great symposium volume: *Man's Role in Changing the Face of the Earth* (edited by W. L. Thomas and others). In its day this was an eye-opener. Now we have come to realize that the changes have often led to environmental deterioration, and that many adverse effects are being accelerated, not least because of uncontrolled population growth. Yet a number of the papers in *The Careless Technology* give some hope for the future. They imply that we often have the knowledge required to integrate our schemes with the environment, but that at present we rarely do so because ecologists do not have the final say. What is needed is a change of motivation toward the environment—an acceptance that we cannot fly in the face of environmental laws. We are going to need all our technological genius to see us through the environmental problems that face us all over the world. Given acceptance of his dependence on ecological laws man may come through but without that acceptance his very genius could be his undoing.

G. W. DIMBLEBY

Reclaimed Parkland



The Fairground State Park, Duquoin, Illinois, is partly reclaimed after strip coal mining. This picture is reproduced (by courtesy of the National Coal Association) in *Power Generation and Environmental Change*, edited by David A. Berkowitz and Arthur M. Squires (MIT Press, Cambridge, Massachusetts, and London, 1971). \$16.95.

Educational Economics

The Political Economy of Education. By John Vaizey. With Keith Norris, John Sheehan and Patrick Lynch and M. F. Leite. Pp. x+297. (Gerald Duckworth: London, May 1972.) £5.45.

THIS book, based on a series of international studies which have been financed by the Gulbenkian Foundation, has already been hailed by the *Economist* as a magnum opus. This should not deter those who are not economists from reading it. Professor Vaizey has the enormous advantage of being an economist who knows what he is talking about when he discusses education; which is more, many educationists would say, than can be said for many economic theorists.

It is good to see the old term political economy coming back into use and the political importance of what Professor Vaizey and his colleagues are trying to do can hardly be exaggerated. Chapter eleven shows that for a range of indus-

trialized countries the proportion of the gross national product devoted to education has roughly doubled in a generation. In the United Kingdom it now exceeds the national budget for defence. In such circumstances it must become very important for politicians to be able both to predict the probable course of educational expansion and to calculate in economic terms the return on public investment from different types of educational expansion.

Professor Vaizey would, I imagine, be the first to agree that even with the help of these studies they are not likely to be able to do either with any great confidence. But that is a condition of social engineering with which politicians have long learned to live. The great value of this work is that it makes a substantial contribution to clearing away errors and misunderstandings, to clarifying thinking about the two great problems and so to enabling planners to plan a little more rationally.

In preferring a "social demand" to a "cost benefit" or a "manpower planning" approach to educational planning in macro-economic terms the authors have not been influenced merely by the enormous theoretical difficulties which they expose in either of the other two, the dangers, for instance, of attributing to factors in education social and economic benefits which arise from quite different causes, or of neglecting the incalculable time-span involved in calculating "returns" on educational "investments", whether public or private. They are deliberately adopting the only economic theory which should be acceptable to a humane and open society. In the long run, when, admittedly, we shall all be dead, a good society will be one which gives all its members as much education as they want and encourages them to demand as much as they need. It is a great merit of the book that the practical difficulties in the process of transition to such an ideal situation are very fully faced in chapter seven.

In the short run, however, and in more micro-economic terms, all this will not help Mrs Thatcher very much in deciding whether to cut back on the costs of higher education in order to finance nursery schools. Students are vociferous in their demand for more; slum children are scarcely even willing to accept it. Yet the needs of the latter, and the social and economic benefits to be derived from meeting them, may be vastly more important. The chapters on the production function and "productivity" in education, where questions of this kind arise, are more tentative, admitting what every educationist would accept with relief in a book on economics, that "the more the objectives of education are examined, the less easy they are to define and to measure".

The production function is itself a

concept of neo-classical economics of doubtful validity, though perhaps of some practical use when applied with caution, but surely scarcely applicable at all where there is such doubt about the possibility of either measuring the extent of outputs or determining their value. Even here, however, politicians could find some ideas worth considering in making decisions which have got to be made, however inadequate the theoretical evidence.

The last two chapters, on teachers, their salaries and recruitment, will be of particular interest to the teaching profession. Perhaps the most interesting revelation is the extent to which the economic practices of productive industry, so doubtfully applicable to education, have been adopted throughout Europe in the form of specified teaching hours and paid overtime. Those who are sceptical of the economic theory which treats the school as a qualified child or manpower producing factory, so often criticized in this wise and liberal book, might reasonably be very cautious about the wisdom of introducing this industrial practice into our more old-fashioned and humane profession.

A. D. C. PETERSON

Science and Scientists

Passion to Know: The Scientists of Today's World—Who They Are, What They Are Doing and Why! By Mitchell Wilson. Pp. xiii + 409. (Weidenfeld and Nicolson: London, August 1972.) £4.75.

MITCHELL WILSON was an American physicist who assisted Fermi in cosmic ray research. He is now a writer, and has written books on science and invention, and several novels. Sir George Weidenfeld, the publisher, proposed that he should travel around the world, visiting the most interesting centres of science, meet the most interesting people, and write a book on what he had heard and seen. The result is a readable mixture of fact, reporting, and opinion. He provides a good deal of interesting statistical information and accounts of the scientific educational situation in the various countries, besides features of research.

The feature he found common to all scientists was the passion to know. This does not, however, reflect the main interest of the book, which is in his accounts of national styles in science. He visited England, the United States, Germany, France, Israel, Russia, Japan, India, and Australia. His book is stronger in its accounts of the contemporary than the pre-war situation in science.

He comments on the influence of class-structure on English science.

Though the old class lines appear to be fading, their ghost may be just as strong. He says that in England the professional engineer has the lowest social status and pay in Europe. British scientists were drawn from a total number of undergraduates which, even after the last decade's expansion, is still a fraction of the number of black students in American colleges. Yet Britain has produced a comparatively high proportion of Nobel prizemen. In interviewing British scientists of working-class origin he was struck by the depth of the psychological marks left by their struggle for education and achievement. He says that the rarest of all the replies he received to his question to scientists, whether there was anything in their background that had led them to science, was from Lord Rothschild. "Why," he replied, "nothing was more natural! I came from a family of scientists!" His father was a zoologist, but Wilson thought that it was difficult for an outsider to feel that seven generations of international bankers can be offset by one generation in the laboratory.

He found that the men over forty seemed to have the proper accent, but with regard to the under forties, a famous Oxbridge FRS told him that "the breaking down of class barriers has become an audible thing now". Associated with this was a lack of confidence in science. In the 1930s a group of socialistically-oriented scientists and writers like "G. C. Crowther, J. B. S. Haldane, J. D. Bernal, Ritchie Calder and Lancelot Hogben" had fervently advocated the use of science as a means of human betterment. The younger generation had turned against their optimistic belief.

The British have not become adjusted in their outlook to their contraction as a world power. A science-trained government economist told him that this country should recognize that it can no longer hope to do, say, four hundred major things. It should concentrate on about sixty, instead of trying to do three hundred, with a sub-critical effort going into each of them.

In boom periods the US spent three per cent of the gross national product on science and technology. Wilson describes the effect of this enormous expenditure on universities, which expand to provide the required scientists. Then the expanded universities attract new science-based industries, because of the opportunities for expert consultation, and for the industrial staff to improve their qualifications by university study. When recession occurs, as in 1970, the process is violently reversed. He provides some additional details to the picture of a huge, complex but unstable scientific-industrial society.

French science is still dominated by the tradition of Napoleonic bureaucratic centralization, which makes it difficult to vary conditions to suit individual talents. He quotes Monod as having serious doubts whether recent attempts to modify it would be successful. The result is a tendency for creative work to be done mainly by those on the fringe of the intellectual bureaucracy, with small resources.

German science in the nineteenth and early twentieth century was regarded by many as leading the world. The contrast with the vacuum under the Nazis and the peculiar features of the post-war reconstruction are described.

Wilson's American cultural presuppositions are even more evident in his account of Soviet than of British science. While he includes interviews with Tamm, Frumkin, and Engelhardt, he does not sufficiently distinguish between criticism and oppression. Wilson conveys the impression that Joffe was

oppressed by the Soviet authorities. In fact he was personally requested by Lenin to train a new generation of Soviet physicists, which he did. Several of the famous institutes mentioned by Wilson were created by Joffe, and many of the eminent Soviet scientists he cites were trained by him. Joffe was a statesman of science, whose influential advice inevitably attracted a number of critics.

One of the problems of Israeli science is the recruitment of scientists from the culturally very varied immigrants.

The sections on Japan and India illustrate the extremely different social and philosophical backgrounds of science in those countries compared with Europe. Consequently their national styles of science appear to us strange.

In the section on Australia an interview with E. G. Bowen, and the rise of radio astronomy, are prominent.

The book contains a number of errors and misprints. The English publisher

might have ensured that Ritchie-Calder did not appear in the index under "Calder" and "Galder". As for myself, I do not care for the initials "G. C.".

J. G. CROWTHER

Politics in Academe

The University and Military Research: Moral Politics at MIT. By Dorothy Nelkin. Pp. xi+195. (Cornell University: Ithaca and London, 1972.) \$7.95; £3.80.

IN 1939, when Charles S. Draper became a full professor of aeronautics, the Massachusetts Institute of Technology was almost entirely financed from student fees and endowment income. During the war it grew big with defence research: \$44 million in 1944-5. Draper's team helped win the war (and a good thing too, if I may say so!) by developing gyroscopic gunsights for aircraft. This work was administered and financed by the standard device of giving research contracts to an "Instrumentation Laboratory" ("I-Lab.") which was legally part of MIT but in practice under Professor Draper's personal direction.

After the war, this convenient legal fiction was perpetuated. Until 1969, all parties to the arrangement were very satisfied. Draper and his colleagues—a technical staff of 700—enjoyed the prestige and personal independence of an academic environment, and went on doing the beautiful precision engineering in which they excelled. The United States Government was getting splendid value, by way of complete inertial guidance systems for submarines, missiles and space systems, for the trifling sum of \$50 million a year. MIT (with an overall budget of \$200 million) was picking up a comfortable percentage of this in overhead costs, staff time for teaching, graduate research projects etc. Many British academics envied this perfect example of the consumer-contractor principle in action.

The boat was rocked by a distant storm—the Vietnam War. Radical or "radicalized" students and faculty began to ask whether universities should undertake military research. The usual happenings, sit-ins, teach-ins, riots, gestures, postures, commissions and so on occurred. At first, it was thought that I-Lab. might be "converted" to more peaceful work like urban transportation; but nobody had the big contract money to offer for that sort of thing—which the I-Lab. engineers didn't care for, anyway. Eventually the president of MIT, Howard Johnson, decided that MIT would "divest" itself of the I-Lab., which could thus go on doing its proper business as a legally independent corporation. This is going to cost MIT

Grubby Shirt in 'Stereoscan'



Epithelial cells adhering to the collar of a cotton-polyester shirt as seen under the scanning electron microscope. The photograph is reproduced by courtesy of 'Stereoscan', Kent Cambridge Scientific, Inc., in *Detergency*, part 1 of volume 5 of the surfactant science series, edited by W. G. Cutler and R. C. Davis (Marcel Dekker, New York, 1972).

a lot of money, though only a small fraction, really, of its gains from this source over the past 25 years.

Dr Nelkin reports this very well, as a case history in the tangled relations of science, technology and society. The more extreme statements and actions of the more extreme parties in this controversy are sickeningly familiar in style; this particular episode is important as one of the biggest in the current phase of academic science politics in the USA. It is certainly very valuable to know just what happened at MIT in that period of crisis.

But to my mind, the discussion of principles does not go quite deep enough. Broad moral issues of pacifism and patriotism are too big and vague. Out of a desperately liberal upbringing, and the experience of a somewhat differently ordered academic system, I would pose two questions.

The first question is: how does one justify an institutional set-up that allows an individual such as Charles Draper to act within it as if he were in personal control of his own corporation, yet bearing none of the risks and responsibilities of a private entrepreneur? Is such liberty of action so technically productive that the price of irresponsibility can be borne—or is it a monstrous abuse of the hallowed principle of academic freedom?

The second question is: must the university always serve slavishly the dominant ideology of society? In the era of the Church, must it be an organ of religion, and in an era of technological militarism must it be given over so largely to military technology? Does it not serve society better, over a longer term, by a deliberate aloofness and separation from immediate ends and means, as a reservoir of the men and ideas that will be needed in a future that will certainly be very different from any that we now imagine?

JOHN ZIMAN

Science Graduates

Graduate School in the Sciences: Entrance, Survival and Careers. By Jack G. Calvert, James N. Pitts, jun., and George H. Dorion. Pp. xii+304. (Wiley: New York and London, August 1972.) £4.50.

What Kind of Graduates Do We Need? Edited by F. R. Jevons and H. D. Turner. Pp. vi+120. (Oxford University: London, May 1972.) £1.10.

WITH the passing of the halcyon days, it is worth re-examining the position of science graduates in society: where can and should they use their expensively acquired education in the context of real or imagined needs? The usual form of such an examination is the

ex-cathedra exhortation, emotionally satisfying but short on specifics. These two books get down to tangible factors that influence individual decisions. It is perhaps significant that both have been written by chemists or ex-chemists, except for one solitary physicist, hard-headed pragmatists to a man.

Graduate School in the Sciences is aimed specifically at the US postgraduate but a lot of its shrewd observations could be used to advantage by the British BSc. It is a postgraduate's Mrs Beeton, for it considers in detail the everyday problems, from choosing the right subject, getting on with the research supervisor, to making decisions about a future career. Perhaps the most important point, which postgraduates and their seniors often forget, is the considerable time-lag between academic choices and their final results. The postgraduate's initial decisions on problem, approach and institution (which will send out the references in the fullness of time) will bear fruit in eight to ten years: his forecasting should therefore be considerably better than that of his government.

Given that we are processing people through the universities in ever-increasing numbers, *What Kind of Graduates Do We Need?* again emphasizes the time scale: "One must accept graduates as they are and with the attitudes of educational changes born twenty to fifty years ago" writes Duncan Davies, general manager for research and development at ICI, who argues for a greater dissemination of science graduates in areas where they could exercise their talents, although not in conventional research and development. More importantly, we should be aware of diversity in needs both personal and societal: Davies remains an optimist although forecasting considerable difficulties on the road to a much better educated society.

Both Davies and H. D. Turner, lately of the British Association and now Secretary of the University of Sierra Leone, are worried by industry's inability to forecast accurately its long term needs and, observes Turner ruefully, "(universities) produce graduates in particular disciplines in numbers which do not bear any adequate relationship to the needs of society and their graduates are not cultivated men in the widest sense of the word". Turner's suggested solution is the encouragement of more general courses somewhat analogous to the Pippard model.

The Pippard idea also enters into C. C. Butler's argument. As Director of the Nuffield Foundation, he is obviously concerned with curricular change and is unhappy about the current way degrees are classified. His tentative solution is a graduate profile given by the university, based on integrated

sciences courses. Once the pattern of courses is brought into play, the problem becomes even more complicated: A. K. Holliday of Liverpool argues against the myth of the over-specialized course: there can effectively be no single-discipline course even under the present honours degree system. Any change must make realistic value judgments about the equivalence of courses.

Between university and industry sits uneasily the university appointments board, represented by Bernard Holloway, Secretary to the University of Manchester Careers and Appointments Service. He recognizes that argument calling for more vocational course content is essentially misplaced and suggests that in the long term the universities should produce 70% generalists and 30% specialists, the former proceeding through what he calls the disintegrated course to reflect the untidiness of the human situation. But whatever the discussion about the precise nature of curricular and institutional changes needed, Holloway, together with most contributors, believes that "arguments for more higher education . . . are arguments for better educated democratic citizens, not for a more highly qualified collection of workers".

In this context the tenor of the contribution by Tony Buley of Unilever, which specifies raw materials with or without added value, goes a long way to explain why some companies find it so difficult to recruit and keep good quality graduates.

P. J. FARAGO

Social Science Policy

Social Science and Government: Policies and Problems. Edited by A. B. Cherns, R. Sinclair and W. I. Jenkins. Pp. xxxiii+269. (London: Tavistock Publications, July 1972.) £6 net.

DISCUSSION of some of the critical aspects of science policy in Britain reached new levels of insularity and intellectual banality with the publication of the Rothschild report. A major change in the power structure of scientific research was made without any apparent awareness of the significance of, for example, the Harvey Brooks report to OECD, or the Lamontagne report on science policy in Canada. Discussion of the objectives of scientific research was abandoned in favour of prolonged exegesis of one particular method of getting research done, the customer-contractor principle.

Social scientists are fortunate that Lord Rothschild appeared to have no taste for looking at the relationship between their research activities and the work of government. It is a complex relationship which demands thoughtful

examination, not least because the principles and processes of government form a substantial part of social science research. Social scientists, if they do their job properly, are likely to be a constant source of embarrassment to government. It requires some sophistication to realize that the general good can be enhanced by paying someone to embarrass you.

Studies of the relationship between social science and government and the formation of policies for the social sciences are still something of an infant industry. Indeed, this book is the first to look at such questions from a variety of different national perspectives. It consists of eleven papers presented at a conference held in Paris in April 1970, under the auspices of the International Social Science Council, together with an extended introduction by A. B. Cherns

and an annotated selective bibliography.

The papers are grouped into three parts, reflecting the conference sessions, entitled "Social Science as a Policy Area", "Aspects of Social Science Policy", and "Social Scientists and the Making of Social Science Policy", respectively. In fact, any one of the eleven papers could have appeared under any one of the three headings without seriously disturbing the book's equilibrium. This comment is not intended as a criticism. It is a comment on the undeveloped state of social science policy as a field of study.

The individual papers are as variable in quality as one might expect them to be. There are, though, some notable high spots. Bertrand de Jouvenel follows a short and lucid treatment of the points of identity and difference

between the natural and social sciences with a caution about the limitations of economic and social statistics. Statistics are a reflexion of past concerns and are of limited value in focusing our attention on the future.

Andrew Shonfield explores the way in which the theoretical consensus underlying economics has enabled economists to play a uniquely important part in the policy-making process, and anticipates the time when the development of a system of social indicators will allow non-economic factors to be given greater weight.

Many conference books suffer from minimal editing. By contrast, this one is edited with care and intelligence. In particular, the "Interchapters", which summarize the discussion after each paper, are models of coherence.

JEREMY MITCHELL

Physical Sciences

Aetherial Problems

The Etherial Problem: a History of the Michelson-Morley-Miller Aether Drift Experiments, 1880-1930. By Lloyd S. Swenson, Jr. Pp. xxi+361. (University of Texas: Austin and London, July 1972.) £4.75; \$10.

WHILE reading this interesting, well-written, thoroughly documented book, I was also reading, collaterally, a book by a hippy about hippies. When I sat down to write this review, what came to my mind's eye was the greatest of them all, walking across the Princeton campus in worn leather jacket, his long white hair breeze-borne, flanked by his two assistants, Bargmann and Bergmann. For if ever a man cocked a snook at the establishment of theoretical physics, that man was Einstein. It was he who converted the then-burning issues treated in this book into dead meat for historians to pick at. In his own estimate, Professor Swenson is three parts historian to one part physicist, and the combination seems well suited to this job. He reconstructs with great skill and sympathy the personalities of his chief characters (Michelson, Morley and Miller). Michelson started as midshipman in the US Navy and was Master when he wrote his paper of 1881 entitled "The relative motion of the Earth and the Luminiferous ether." Swenson writes: "Michelson was a sailor, pilot, and navigator before he became a physicist. His greatest fame as a physicist stems from his efforts to solve certain kinds of relative-motion problems which he could not have avoided as a naval

officer." The paper in question is reproduced in an appendix and I started to read it with the reverence due to an historical document. Can you believe it? This naval officer was apparently ignorant of the parallelogram of velocities, and his calculations err by a factor of two. In the more famous Michelson-Morley paper of 1887 this "oversight" was corrected, having been pointed out by Potier and Lorentz.

It is interesting to compare the dates of Michelson (1852-1931) with those of Edison (1847-1931). This was the great American age of what might be called, without disrespect, inspired gadgetry, and Michelson's interferometer was a brilliant example of such creative skill. The visible spectrum of physics (the invisible part lying in the subconscious) extends from accurate experimentation to the creation of new concepts, and it was in the former that Michelson, Morley and Miller excelled. Their concepts were basically the absolute space and time of Newton, associated with a mischievous aether which refused to behave consistently. The date (1905) of Einstein's famous paper neatly bisects the time-range of the aether-drift experiments described in this book, and one might expect relativity to have killed them off earlier. But we read of Einstein visiting Miller in 1921 and urging him to continue aether-drift trials and to re-examine the earlier results obtained in 1881, 1887 and 1902-1905. Perhaps it is not as simple as it seems. To teachers of physics I suggest as a healthy exercise the preparation of a lecture to explain, clearly and succinctly, how one can accept,

consistently, both the null result of the Michelson-Morley experiment and the facts concerning stellar aberration, said lecture to be delivered, not to a herd of sheep, but to a class of intelligent undergraduates with a minimal respect for the authoritative word.

When I accosted relativity in 1920-21, that was one of the problems I found difficulty with, but I had the good fortune to attend the course of lectures by Silberstein in Toronto, mentioned in a footnote by Swenson (page 197); I must, however, correct a detail—Michelson was not there, to my knowledge. Silberstein not only lectured excellently, he explained things to me personally; nevertheless certain difficulties remained, notably with regard to rigid bodies. It took me many years to realize that all this talk about measuring rods is bunkum, as Michelson himself ought to have seen, since, in an interferometer, the things compared are not lengths, but optical paths, that is times.

According to evidence presented in this book, the Michelson-Morley experiment was not, as is sometimes supposed, "the primary cause and justification for Albert Einstein's first work on the theory of relativity." As for the small positive results reported by Miller in his aether-drift experiments, these are (in view of the elaborate analysis by R. S. Shankland *et al.*) to be dismissed as thermal effects.

There is a bibliography of fifty-seven pages, appendices reproducing papers by Michelson and Morley, and a good index. Footnotes are where they should be, at the bottom of the pages.

J. L. SYNGE

Materials

Treatise on Materials Science and Technology. Edited by H. Herman. Vol. 1. Pp. xii+346. (Academic: New York and London, 1972.) \$18.50.

My dictionary defines "treatise" as: "1. A book or writing that treats of some particular subject. 2. A formal and systematic exposition in writing of the principles of a subject, generally longer and more detailed than an essay." The volume under review contains much valuable matter, but it is not a treatise. This needs saying at the outset, because the lines of demarcation in scientific writing are becoming ever more blurred. There is a continuous spectrum from true textbooks or treatises, via occasional monographs (including individual review essays), collections of reviews published at regular intervals such as progress volumes, review articles in research journals, conference proceedings, to regular research papers in research journals. Information retrieval, difficult at the best of times, becomes almost impossible when the categories are confused: a volume like that here discussed is in effect a collection of reviews, and from internal evidence will be followed by further volumes of the same kind; yet it is presented misleadingly as a treatise and will be tucked away in libraries along general textbooks (since it cannot be catalogued under any of its constituent specialist topics), and will rarely be found by those who need it. A periodical, of whatever character, should be frankly a periodical.

This said, I can warmly recommend most of the individual essays in this book. They are written at a high and consistent academic level: it is only the width of appeal that varies. Tiller opens with an exceptionally learned and interesting survey of the structure and physical chemistry of interfaces; he analyses the constituent terms of interfacial energy and relates this to the kinetics of processes such as nucleation and the development of crystal morphology. Argon follows an earlier initiative of Rosen and examines the fracture of composites in terms of a Weibull-type statistical distribution of defects. Tewary and Bullough include a very thorough (and difficult) analysis of the propagation of ultrasonic waves, of wavelengths close to the interfibre spacing, in fibre-composites, pointing out the structural information to be so derived; however, the approach is so fiercely theoretical that we never learn what variables are to be measured.

Hasson and Arsenault show how evidence can be obtained about the strength of interactions between interstitial and substitutional solutes in body-centred cubic metals by examining the modifications in the Snoek internal friction peak as substitutional solute content is varied.

The article is novel and the technique promising; moreover their essay finishes with a particularly clear summary of the advantages and limitations of the technique. De Hoff contributes a mathematically deep analysis, in terms of quantitative metallography, of the links between changes in particle size, interfacial area, triple lines, etc., in both simple systems and complex ones, where impingement takes place. This essay is open to the criticism that no practical illustrations whatever are given, whereas (for instance) the equally deep essay by Tiller is made easier to follow by many applications. Finally, Haskell and Byrne describe their own work on chemical vapour deposition: this essay, being based on a single research programme, lacks the breadth of the others.

I should like to conclude with an appeal to some enterprising publisher to produce, soon, a catalogue and index of those review articles of all kinds in the broad field of materials science which have been published, say, in the past 7 years; treatises, progress series, monographs and learned periodicals should all be included. The profession badly needs this service. R. W. CAHN

Thermodynamics

Physical Chemistry: an Advanced Treatise. Vol. 1. *Thermodynamics.* Edited by Henry Eyring, Douglas Henderson and Wilhelm Jost. Pp. xxii+659. (Academic: New York and London, June 1971.) \$29.50; £13.75.

SEVERAL volumes of this advanced treatise have already appeared and have been well received. The present volume deals with thermodynamics. It is, however, neither a "basic" volume for the whole treatise nor a unified survey of modern thermodynamics: it is a somewhat heterogeneous collection of essays and reviews which, while containing much excellent material well presented, does not integrate into a book which the reader is encouraged to read from cover to cover.

The book begins with a survey of fundamental laws, by R. Haase, which is mainly an account of classical thermodynamics. By using the concept of generalized work, the formulation is in principle applicable to systems in anisotropic strain states, and subjected to external fields. An introduction is given to "local thermodynamics" which is important in the discussion of continuous systems in which the values of the intensive variables vary with position in the system. No two discussions of the first and second laws of thermodynamics have the same emphasis, and it is always instructive to read a different presentation: a few pages deal with Carathéodory's method to which a whole chapter is devoted later. A modernized

form of the Nernst Heat Theorem is developed; the third law of thermodynamics is mentioned, but it is recommended that neither this, nor the principle of the unattainability of the absolute zero, should be accorded the status of a "law".

Chapter 2 (by A. Sanfeld) consists of three parts, A devoted to "Equilibrium, Stability and Displacements", B on "Irreversible Thermodynamics", and C on the "Thermodynamics of Surfaces". All three sections call heavily on the work of the Brussels school. A is largely a précis of *Chemical Thermodynamics* by Prigogine and Defay (Longmans, 1954) and makes extensive use of the examples and reproduces many of the figures from this book. It introduces little new but, regrettably, reverts to the old Brussels non-IUPAC notation for several thermodynamic quantities. Part C is again based largely on *Surface Tension and Adsorption* by Defay, Prigogine, Bellemans and Everett (Longmans, 1966), but includes new work on the slow orientation of molecules at an interface. One section is an almost exact reprint from this book. Part B is a brief and hardly adequate introduction to irreversible thermodynamics.

Chapter 3 is a useful summary by R. Haase of the thermodynamic properties of gases, liquids and solids, including ionic melts.

Chapters 4 and 5 deal with the special problems of equilibria at high pressures (up to about 4kbar) (by E. U. Franck) and in external fields (by H. Henschke) and are recommended. In spite of the plea in chapter 1 that the name "third law of thermodynamics" should not be used, chapter 6 (by J. Wilks) bears this title. It covers very much the same ground and reproduces verbatim many passages from the book of the same name by this author (Oxford, 1961).

Chapter 7 (by Max Klein) on "Coupled Gas Equilibrium" deals almost entirely with the mathematical and computational problems associated with the location of free energy minima in multivariant systems, and provides an important link with modern applications of thermodynamics to chemical industry. Chapter 8 (by H. Kremp) on "Equilibria at Very High Temperatures" would have been more appropriately called the "Thermodynamics of Plasmas": the present exposition of a developing field is useful and timely. The following chapter 9 (by R. H. Wentorf) on "High Pressure Phenomena" is largely concerned with pressures greater than 20 kbar. It has a strong experimental bias and gives a good impression of the problems which make this such a difficult field. The final chapter (by S. M. Blinder), already mentioned, is essentially a plea that, now students of chemistry, physics and engineering have a "continually in-

creasing level of mathematical sophistication", it is appropriate to consider presenting the second law of thermodynamics to them through Carathéodory's formulation; and this chapter outlines the way in which this can be done. Interesting and stimulating though this is, I wonder whether it really is appropriately included in this treatise; and, if so, why it should have been relegated to the end.

In all, I approach this book with mixed feelings. There is a need to bring together the various branches of physical chemistry and in particular to point the way to new applications of thermodynamics. And because few scientists today have the time to write encyclopaedic textbooks, multi-author volumes of the present kind seem inevitable. Yet when they do little more, as in parts of this volume, than to reproduce the core material of standard textbooks, I wonder whether an annotated reading list would not have been almost as useful—and would have left more room for a fuller treatment of more sophisticated topics: it is for this material that the book will be most useful.

The production is excellent, as we expect from this publisher, and few errors (except the interchanging of Figures 17 and 18 of chapter 2) were noted.

D. H. EVERETT

Crystallography

Methods in X-ray Crystallography. By J. W. Jeffrey. Pp. xxix + 571. (Academic: London and New York, December 1971.) £11.50; \$35.

ACCORDING to the jacket, the main aim of this book is to cover the techniques required for the interpretation of single-crystal X-ray diffraction in detail, up to the production of observed structure factors for use in structural analysis. The book can be divided into four approximately equal parts; the first develops the basic theory, the second describes the construction and use of every kind of X-ray camera, the third consists of ten appendices (including one on radiation safety devices), and the fourth is devoted to forty-two exercise examples and their detailed solutions.

A more accurate title would be "Photographic Methods in X-ray Crystallography", for the emphasis throughout is almost entirely upon photographic techniques. The more commonly used counter methods for intensity measurement (in a recent number of *Acta Crystallographica*, Section B, about 70% of the structures were determined with counters) receive only one or two rather fleeting references. Professor Jeffery is well known as a devotee of photographic methods,

and his enthusiasm and flair come over strongly. The reproductions of all kinds of X-ray photographs are quite superb. The student would take a very long time to be able to produce photographs of this high quality, but the book certainly tells him all he could possibly need to know in order to do so.

The needs of students new to the subject are well catered for by the inclusion of "Students' Notes" (which provide a guide to the use of the book), a comprehensive and definitive list of symbols, and appendices covering basic mathematics. But Professor Jeffrey has possibly been a little over-solicitous in places—for example in the passage dealing with centring of crystals on camera axes.

The bibliography is rather scant, there being only twenty-two references to books and papers. Inevitably in a work of this magnitude, there are a few minor blemishes such as wrongly numbered equations, incorrect references to appendices and the consistent mis-spelling of Cochran. But the presentation is attractive, the treatment sympathetic and the work should be of considerable benefit to students.

J. P. G. RICHARDS

Scattering of Atoms

The Calculation of Atomic Collision Processes. By Kenneth Smith. Pp. x + 218. (Wiley: New York and London, April 1972.) £5.50.

Scattering Theory: The Quantum Theory on Nonrelativistic Collisions. By John R. Taylor. Pp. xvi + 477. (Wiley: New York and London, August 1972.) £6.75.

A WELCOME addition to the literature, Professor Smith's book should be of particular use to the postgraduate student interested in atomic-collision theory. It is divided into Part 1, an introductory course, and Part 2, a more advanced course which might benefit any active researcher in atomic physics.

Although one calculates numbers as against processes, the title is accurately descriptive. The author's conviction is that models in theoretical physics must be put to the quantitative test. The emphasis on the algebra and numerical analysis of the algorithmic approach is appropriate, whether to electron-atom, atom-atom or electron-molecule collisions. Taken in conjunction with *Computer Physics Communications*, the student is presented with ample scope for experimental projects on the computer.

Of course, physical understanding does not always come from studying heavy algebra: the physics is often to be found in the various complex planes. The physical interpretation of zeroes and poles in the complex energy (or linear-momentum) plane therefore strikes a

Candled Ice Surface



Crystalline structure of ice on Peters Lake, Alaska. Where the ice looks light coloured, the *c* axes of the crystals are vertical; dark ice has horizontal *c* axes. The photograph, originally published by the International Glaciological Society in the *Journal of Glaciology*, is reproduced in chapter 12, "Fracture of lake and sea ice", of *Fracture: An Advanced Treatise*, edited by Harold Liebowitz (Academic Press, New York and London, 1972). This volume, Volume 7, deals with nonmetals and composites.

balance. On the other hand, the treatment of branch points is less clear. For instance, the section on Coulomb scattering omits any specification of branch cuts, so that one must read equation (1.54) as an act of faith. In contrast, the discussion of the multivalued nature of fourth-order Runge-Kutta procedures is excellent. It is, perhaps, understandable that the complex angular-momentum and particle separation planes are neglected, as are such unifying concepts as unitarity, analyticity and symmetry.

Perhaps the reader should be warned that computer time is finite and that the art of heavy-particle, as against electron-atom, collision spectroscopy is often best served by first reducing the close-coupling equations from second to first order by judicious use of the JWKB approximation. Although much has been published in the two years since the most recent of the cited references, many should find the book valuable, if slightly expensive.

In the preface of his book on scattering theory Dr Taylor expresses the hope of bringing the student of atomic or nuclear physics to the point where he can begin reading the literature and tackling real problems, with a complete grasp of the underlying principles. Provided it is understood that literature in this context refers not only to the journal literature but also, with priority, to the specialist atomic or nuclear texts, some of which appear in the bibliography, then this hope would appear not unduly optimistic. The emphasis on formal scattering theory is certainly appropriate to the nuclear or elementary-particle physicist. The atomic-physics student will find the book complementary to that of Dr Smith and will probably wish to browse from chapter 10 onwards, omitting the formalities of time-dependent theory. The sections dealing with the complex linear- and angular-momentum planes should be found particularly rewarding, as should those on invariance principles. Perhaps the reader might have preferred discussion of uniform convergence in connexion with Gauss convergence factors and Coulomb scattering, of momentum transfer factors in heavy-particle charge-transfer rearrangement collisions, and so on. Indeed, he might even object to some statements, for instance in the introduction, that the computation of the scattered and incident fluxes completely ignores the interference of the two waves; in fact the total flux density is given by

$$\left\{ p - \frac{4\pi}{r^2} \delta(\hat{\mathbf{x}} - \hat{\mathbf{r}}) \operatorname{Im} f(E, 0) \right\} \hat{\mathbf{x}} + \frac{p}{r^2} |f(E, \theta)|^2 \hat{\mathbf{r}} + O(r^{-3})$$

from which the usual results follow. Despite such quibbles, the more mathematically minded will in general find this a challenging and scholarly treatise well worth the expense and the effort of working through the reinforcing problems appended to each of the twenty-two chapters.

D. S. F. CROTHERS

Reactions in Solution

Mechanisms of Homogeneous Catalysis from Protons to Proteins. By Myron L. Bender. Pp. x+686. (Wiley: New York and London, December 1971.) £11.75.

FROM a chemist's point of view, the processes of life are chemical reactions proceeding in aqueous or partly aqueous phases. The chemist's contribution to their understanding has therefore, in the first place, been directed towards the study of simpler reactions in aqueous solution, on the assumption of the apparently self-evident axiom that the behaviour of complex systems cannot properly be rationalized before that of simpler systems. It has been a part of this general approach to consider the mechanisms of reaction *in vivo* in terms of *in vitro* mechanisms of simple reactions of organic substrates in solution. Probably all reactions in solution are subject to catalytic effects, and the remarkable catalysis by proteins ("enzymes") is thus seen by the chemist as a complex problem of qualitatively the same type as simpler and less effective forms of catalysis.

Professor Myron Bender's new book is a masterly exposition of this point of view. It traces the developments which have taken students of reaction mechanisms from topics such as the saponification of ethyl benzoate to the investigation of acetylcholinesterase and lysozyme catalysis. There have been a number of interesting recent books devoted to the chemical approach to enzyme action. Professor Bender's contribution is remarkable for its careful treatment of the basic physical chemistry of simpler catalytic phenomena, which is clear without conveying the wrong idea that all simple problems have been "solved". It then guides the reader from the securely established basic phenomena to problems of obvious complexity. The progression is convincingly logical, with the aim of embracing the entire field and considering the treatment simply as an updating of R. P. Bell's classic monograph *Acid-Base Catalysis*. The main stream of the development is that from oxonium and hydroxide catalysis, via general acid-base, nucleophilic and electrophilic catalysis, metal ion effects, bifunctional and multifunctional catalytic phenomena and neighbouring-group effects in model compounds, to enzyme mechanisms. Confluent contributions, such as

investigations of enzyme structure and of enzyme-substrate binding, are likewise traced. With his prodigious knowledge of organic and physical chemistry, Professor Bender is able to present an elegant and impressive chain of arguments. The reader may not agree in detail with every one of the examples adduced in this exposition, but he cannot fail to find stimulation and a clearer perspective of scientific development. The actual treatment of enzymes and their catalysis takes up about 260 of the approximately 660 pages of solid and well-documented text.

The book was written over a period of five years, with a lengthy interruption due to the author's illness. This extended period appears, if anything, to have resulted in a more mature balance of material, and has not produced the lack of unity often observed in such cases. The book will be studied with pleasure and profit by students of physical organic chemistry who are venturing into the rich country of enzyme action. It can also be recommended to biochemists and biophysicists who have approached enzymes from a more phenomenological or structural point of view and who may need more persuasion to see the essential unifying link of catalysis between simple reactions in solution and enzyme action.

I hope that a cheaper edition of this book will soon bring it within the reach of students' pockets. V. GOLD

Turbulent Models

Turbulence Phenomena: An Introduction to the Eddy Transfer of Momentum, Mass and Heat. By J. T. Davies. Pp. xi+412. (Academic: New York and London, June 1972.) \$19.50.

Mathematical Models of Turbulence. By B. E. Launder and D. B. Spalding. Pp. 169. (Academic: New York and London, July 1972.) £2.50; \$7.50.

THE central problem of turbulence is the unclosed nature of the relations derived by statistical averaging of the Navier-Stokes equations. In the past, this difficulty has been avoided either by outright correlation or by closure at the lowest level. This is done by assuming that the Reynolds stress is a functional of the mean velocity gradient (e.g. the mixing length method). Correlations cannot be used to predict anything; the mixing length method is fundamentally wrong, and has been pushed for rather more than it is worth.

It has long seemed likely that raising the level of the closure would greatly improve the performance of turbulence models, but this possibility remained theoretical until large digital computers became generally available. The com-

puters are needed, not only to solve the equations of motion, but also to choose the arbitrary constants in the equations of motion by best-fitting of the experimental data.

Within the last few years it has become clear that these models are indeed greatly superior to the earlier ones. The clear and authoritative book of Lauder and Spalding describing these developments, is therefore timely. It is based on lectures given by the authors at Imperial College. The slides used in these lectures, which contain basic statements, graphs or equations, are reproduced in a box at the head of each page. The verbal discussion of each slide is reproduced as text below the box.

This unusual format enabled the authors to get the book out very quickly so that it is up to date although the subject is developing very fast. It does demand unremitting concentration from the reader. If he is willing to make the effort, he will be rewarded by rapid understanding of a diffuse and difficult subject.

The exposition starts with the old eddy viscosity and mixing length models, and gives at least some space to every turbulence model which has been seriously proposed for practical work. The numerical constants which should be used in the models are quoted whenever they are known, and the model predictions are extensively compared with experimental data. Apart from its primary purpose, the book is a mine of references to the best experiments over a wide area of fluid mechanics and heat transfer problems.

The final chapter is called "Probable future developments". It anticipates the application of the model to such things as (atmospheric) cyclones and industrial furnaces, but it also tries to forecast the development of the models themselves. The forecast is of more of the same—one-point closures with constants determined from experiment. I like to think that one day we shall be able to do more than this.

When a mechanical engineer thinks he has fluid flow problems, it is chastening for him to look at the range and complexity of the situations which concern a chemical engineer. Professor Davies describes a variety of such situations. His main theme is mass transfer, and he gives particular emphasis to surface effects in two-phase flows. The book moves from one phenomenon to another, giving both description and theoretical interpretation.

Some of the descriptive material is very good, and the supporting photographs are excellent. The theoretical material is very much less satisfactory. The depth of the exposition inevitably varies, reflecting the uneven state of our knowledge. When the author does ex-

pound in depth, the mathematics is ponderous and the end result is often badly out of date. I was particularly shaken to find that the most recent theory of heat transfer to a solid surface mentioned in the book is that of Prandtl (1910).

The book would mislead a novice on certain important points. However, it contains interesting material which is not readily available in book form, and a mechanical engineer who wanted to broaden his vision might well find it helpful.

D. C. LESLIE

Kinetic Mathematics

Kinetic Systems: Mathematical Description of Chemical Kinetics in Solution. By C. Capellos and Benon H. J. Bielski. Pp. xii+138. (Wiley: New York and London, August 1972.) £5.

THIS little book has been written primarily for chemists needing guidance in the mathematical techniques essential to kineticists. Its introductory chapter concludes with references to five standard textbooks on chemical kinetics. Most of the content of this monograph is available in them, usually as appendices. This fact by no means renders the present booklet superfluous. Many scientists will doubtless benefit by its compactness. Nevertheless, the complete absence of data is a fault which, one hopes, will be rectified in later editions. Consider, for example, reactions of kinetic order of five or six. To mention these numbers would be not only true and relevant but would be as jam with the powder to titillate the digestion of much that is old and inevitably stodgy.

The notation, though commendably precise, is over-elaborate, and must have given headaches to compositor, proof-reader and printer alike. Mature kineticists will inevitably toy with the possibility of using the first three letters of the alphabet to denote constant compositions, and the last three letters to denote time-dependent compositions, all without superscripts, subscripts, square brackets and other frills.

Insufficient attention has generally been given to the limitation of the applicability of some of the kinetic expressions used. For example, Debye's treatment of the kinetics of reactions between ions in solution, which is reproduced in full, adopts a macroscopic permittivity, a semi-empirical distance of nearest approach, and is restricted in its validity to a single encounter in an infinitely dilute solution.

Omitted from a wide range of kinetic systems is that of the ammonium cyanate-urea reaction—a reversible second-order and first-order reaction in a time-variable ionic environment. It is historically important, biologically interesting and kinetically challenging.

E. A. MOELWYN-HUGHES

Karst

Karst. By J. N. Jennings. (An Introduction to Systematic Geomorphology. Volume 7.) Pp. xviii+252. (MIT: Cambridge, Massachusetts, and London, January 1972.) \$8.95.

Karst: Important Karst Regions of the Northern Hemisphere. Edited by M. Herak and V. T. Stringfield. Pp. xiv+551. (Elsevier: Amsterdam, London and New York, 1972.) Dfl. 75; \$22.

THESE two books, bearing similar titles, cover a subject concerned with terrain that has distinctive characteristics of relief, drainage, and other features that arise from the solubility of rocks. Bedrock consisting of limestones, and less commonly of evaporites, such as salt or gypsum, develops in humid climate a topography of chaotic relief with closed depressions, underground streams, large caves, and dry stream valleys. These features result from solution of the bedrock by running fresh waters, including percolating phreatic waters that enlarge existing voids in the rock. The result of progressive dissolution is increased permeability that finally yields a capacity to transmit large amounts of water underground, hence giving rise to dry valleys.

The word karst derives from a German transliteration of the Slav word *Krš*, meaning crag or stone. The classic site of karst is in the Dalmatian Mountains of Yugoslavia, hence the Slav and German languages enter into the meaning and definition of this specific landform.

The book by Jennings is an introduction to the geomorphology of karst. The book edited by Herak and Stringfield is a multi-authored effort in which experts from countries in the northern hemisphere wrote chapters on the karst features in their areas of study. Neither book is complete. Jennings's bias is towards karst in Australia, New Zealand, New Guinea, and Malaysia, areas with which he has personal acquaintance. The classic karst regions of Europe, North America, Java, and the Caribbean receive less attention than they deserve, or no attention at all. The book edited by Herak and Stringfield has eleven chapters on Europe, one on the United States, and one on Jamaica. Hence the lack of geographic coverage in one book is partly compensated by the other.

Both books, each in its own way, have much to offer to geologists and geographers interested in landforms, particularly to those scientists with interest in the behaviour of limestone terrain subjected to running water. Non-specialists will be frustrated by the use of less common terms, such as dolines, poljes, hums, ponors, uvaes, cockpits, grikes, tafonis, and clints, to name but a few. A glossary would have been a

most valuable addition in making these books more useful. Both books deal with modern karst as presently exposed on the surface of the Earth. In the rock record many ancient karsts form unconformities at the tops of limestones. Although such palaeokarsts may be important economically, for instance as traps for oil, gas, lead or zinc, they are not covered in these two books.

The chapter headings of Jennings's book include nature of karst and its study, karst rocks, karst processes, minor solution sculptures, drainage, surface landforms, karst caves, cave deposits, influence of climate and rates of denudation, influence of geologic structure, historical geomorphology of karst, and present state of karst geomorphology and its value. The book is well written and well planned. Some of the material that is covered in this book leaves one hanging. Why, for instance, has Jennings reviewed several petrographic classifications of limestones when nowhere in this book is a relationship shown between the fabric of the limestone and the development of karst?

In addition to the chapters on karst regions the book edited by Herak and Stringfield includes two historical chapters and a joint chapter by the editors reviewing general concepts. As the Anglo-American literature has not loomed as large in this field as in most other fields of geology, Anglo-American students of karst will be grateful to the authors of these chapters who have made available in the English language much information which lies behind a very real language barrier. As the original manuscripts were written by scientists from several different language groups, the writing and presentation are uneven. The true student in this field, however, overlooks this disadvantage; he realizes that it is unavoidable.

GERALD M. FRIEDMAN

Rock Mechanics

Rock Mechanics and Engineering. By Charles Jaeger. Pp. x+417. (Cambridge University: London, August 1972.) £10.50; \$25.

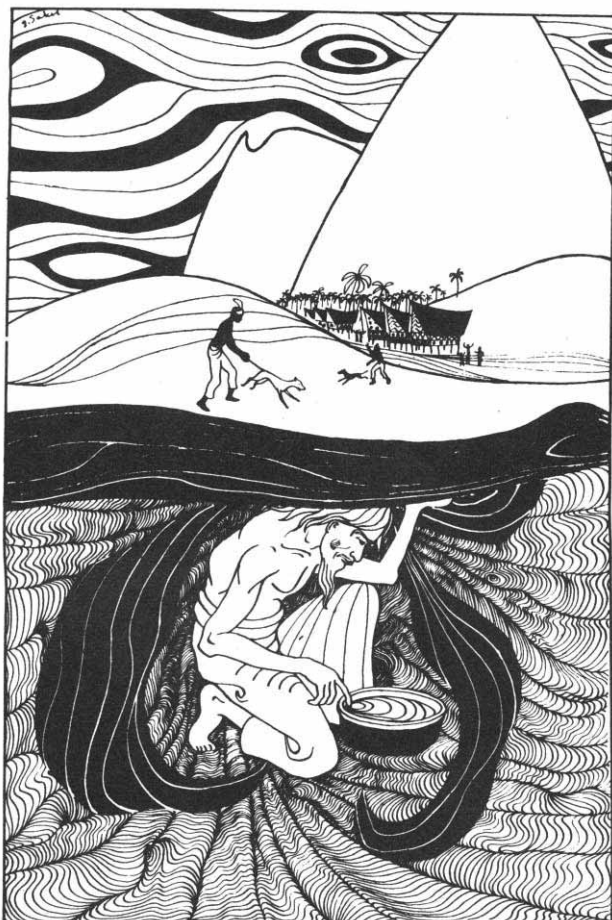
UNTIL 1967 there was no authoritative

textbook in English on the new science of rock mechanics. Since then a number have appeared and this latest, by Dr Charles Jaeger (not to be confused with his Australian namesake, working in the same field), is a valuable addition to the library of the engineering geologist and the civil engineer who are concerned particularly with the field of large dams or underground openings.

Dr Jaeger paints his canvas with a big brush. In the fourteen chapters of the book we start with a history of how rock mechanics has developed and end with a case history of the disastrous 1963 Vaiont rock slide in Northern Italy which displaced the water from the freshly filled reservoir and in turn caused the death of nearly 3,000 people around the small town of Longarone. The other sections of the book deal with rock material and rock masses, the stability of rock slopes and dam foundations, and the stresses in tunnels. These are the core of rock mechanics.

It is difficult for a book to be everything. Any one of the sections in the stress analysis portion could be enlarged and treated in greater detail, as for example the finite element method which is given only five pages. Nevertheless the book gives an excellent overall view of the state of rock mechanics today from an engineer who has been closely involved with theory and practice. Many of the references do not appear to have been checked: thus "Professor Bjerrum representing the Chairman of the International Conference on Soil Mechanics" should in fact have read "Professor Bjerrum, Vice-President for Europe of the International Society for Soil Mechanics and Foundation Engineering", and Arthur Casagrande is consistently referred to as H. Casagrande. KEVIN NASH

Roof of the Earth



Maradika Pudu, Indonesian Spirit of the Earth, as drawn by I. Sakri. When he puts his finger in the bowl of water, an earthquake results. Reproduced as the frontispiece of *The Upper Mantle*, Volume 4 of *Developments in Geotectonics*, edited by A. R. Ritsema (Elsevier, Amsterdam, 1972). The book describes, among other things, some alternative explanations of earthquakes.

Slope Forms

Slopes. By Anthony Young. Edited by K. M. Clayton. Pp. vii+288. (Oliver and Boyd: Edinburgh, March 1972.) £5.

Hillslope Form and Process. By M. A. Carson and M. J. Kirkby. Pp. viii+475. (Cambridge University: London, June 1972.) £6.60; \$19.50.

GEOMORPHOLOGY is gathering unto itself an ever increasing body of specialist literature. Now two books have appeared almost simultaneously which from their titles would seem to cover the same general ground. The similarity, however, largely ends at that point.

Dr Young's is a well conceived and ably presented review of much of the important work done on slope analysis, with a special emphasis throughout on the explanation of slope form. Some

of the more elementary concepts of rock and soil mechanics are included where they are relevant to the analysis of materials or of slope stability. Dr Young is careful to provide a conceptual framework (such as the systems approach) for the ideas which he presents. The last chapter in this book outlines the relevance of slope studies to engineering, agriculture and soil science, and in so doing Dr Young moves into one of the growth points of the subject, namely applied geomorphology.

Professors Carson and Kirkby adopt a more direct approach to slope form studies through a synthesis of quantitative work in the study of processes and materials. This carries these authors much further into the fields of rock and soil mechanics than is the case with the book by Young. The necessity is seen by Carson and Kirkby for evaluating the dynamic systems of slope processes under different climates by giving them separate treatment in four chapters on humid temperate, periglacial, semi-arid and arid, as well as tropical areas. The concluding chapters of their book are concerned with the establishment of theoretical models of the development of slopes in profile, as well as with respect to the situation of slopes within drainage basins. This continues the welcome and refreshing break, adopted by the authors throughout the book, from a merely statistical to a fully mathematical approach to landform studies. It also makes an interesting contrast with Young's concluding emphasis on the practical applications of slope studies.

There are other contrasts apparent between these two books. The series of geomorphology texts to which Dr Young makes this contribution is aimed at the undergraduate market, while Professors Carson and Kirkby direct their work towards the graduate student. This contrast in level is clear as both books succeed in their stated tasks. Thus to see them as being complementary to each other is not quite fair as they are written for different audiences. Yet it is not possible to read both books without being doubly stimulated by their complementary approaches. Here are two very good books.

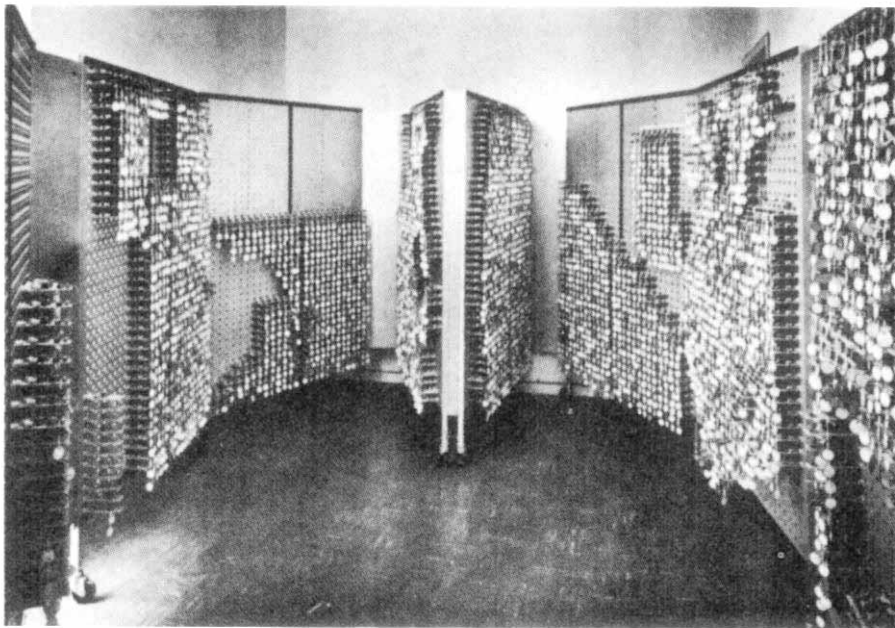
J. C. DOORNKAMP

Flow Through Soil

Advanced Soil Physics. By Don Kirkham and W. L. Powers. Pp. xv+534. (Wiley: New York and London, May 1972.) £10.

DON KIRKHAM is well known to the soil scientist for his numerous papers on seepage of ground water into ditches

Chesapeake Bay Analogue



Computer analogue used to investigate the responses of the Chesapeake Bay waters to artificial disturbances. A method described in one of the papers comprising Volume 10 of the annual review of *Oceanography and Marine Biology*, edited by Harold Barnes (George Allen and Unwin, London, 1972).

and drains and partially penetrating wells. This book, with W. L. Powers, puts these many papers into perspective. After a short introductory chapter on static water in soil we are led straight to Darcy's law, the Laplace equation and the various artifices and techniques used to solve ground-water flow problems. The Schwarz-Christoffel transformation is introduced, and as an example of its use the flow at a corner and into a ditch is analysed. A further chapter deals with unsaturated flow, and J. R. Philip's solution for horizontal and vertical infiltration is described in detail. The final chapters deal with miscible displacement, gaseous diffusion in soils and soil temperature. The book is mathematical in its approach, the more difficult expressions being derived in detail. Each chapter is provided with a set of exercise problems, and there is a good bibliography and list of references.

From this it will be seen that Kirkham and Powers deal primarily with the physics of flow through porous media, and the title they have selected, *Advanced Soil Physics*, is therefore somewhat abbreviated. Soil consists of the soil skeleton, plus water and air in the voids, but this book contains almost nothing about the former as might have been expected from the title. It is likely to be of greatest value to research workers in soil physics, hydrology and agricultural engineering who are concerned with ground-water flow, and these will find it excellent.

KEVIN NASH

Vibrational Spectra

Infrared and Raman Spectra of Crystals. By George Turrell. Pp. xii+384. (Academic: New York and London, September 1972.) £8.50; \$26.50.

ADVANCES in laser Raman spectroscopic and infrared techniques have led to a rapid increase in the number of papers dealing with the infrared and Raman spectra of crystals. Hence this textbook is very timely especially as books by previous authors have considered the dynamics of isolated molecules rather than the cooperative effects which occur in condensed phases. This new guide by Professor Turrell which discusses the interpretation of vibrational spectra of crystalline solids is therefore the first work to discuss the subject in its entirety. The text has grown out of a series of lectures on the theory of vibrational spectroscopy which the author presented at the University of Bordeaux between 1968 and 1970.

The first two chapters review the dynamics and symmetry of molecular vibrations and introduce the matrix formulation which is used throughout the book. For readers who are familiar with the theory of molecular vibrations in isolated molecules, these chapters merely serve to define the notation that is used throughout the book. However, those who are studying the theory of molecular vibrations for the first time should find here the basic material for understanding the later chapters. Little mathematical background is needed

beyond elementary differential equations and the fundamentals of matrix algebra.

The dynamics of crystal lattices and the theory of space groups which are developed in chapters 3 and 4 provide the crystallographic basis for the interpretation of the vibrational spectra of crystals. Factor group analysis is derived from first principles and illustrated using a few examples of both molecular and ionic crystals. The Hermann-Mauguin and Schönflies systems of group notation are used together to help crystallographers and molecular spectroscopists to understand each other. The next chapter continues with the introduction of classical electromagnetic theory to treat the interaction of crystal vibrations with electromagnetic radiation.

In the final three chapters, applications of vibrational spectroscopy to the determination of potential constants and structures and to the study of polymer systems and impure crystals are considered. The methods presented for the vibrational analysis of polymer chains and crystalline polymers should prove useful to workers in the field of polymer science and biochemistry where spectroscopy can provide a considerable amount of structural information.

The final chapter emphasizes the spectroscopic similarity of doped crystals and matrix isolation systems. In both cases the vibrational spectra are characterized by the effects of guest-host-lattice interactions and "localized" lattice vibrations. The addition of a number of valuable tables, as appendices, should increase the practical use of this book for the interpretation of the vibrational spectra of crystalline solids.

Professor Turrell has written a most helpful book which should appeal to very many chemical and physical spectroscopists. The book is a real contribution to the spectroscopic literature and no solid state spectroscopist can afford to be without it. This is a book that we have needed for a long time and for which much gratitude is due to its author.

G. R. WILKINSON

Sliding Solids

Friction and Lubrication of Elastomers. By Desmond Moore. Pp. xvii+288. (Pergamon: Oxford and New York, June 1972.) £7.50.

THE friction between sliding solids arises from two main causes: adhesion at the interface and the deformation of one surface by asperities on the other. Twenty years or so ago it was shown that in the sliding of hard asperities over rubber the deformation component of the friction could be calculated in terms of the viscoelastic losses in the rubber.

A little later, experiments showed that the adhesion component was also a function of the viscoelastic properties of the rubber though the time scale was appreciably different. These experiments led to a "unified" approach to rubber friction along two main lines; an atomic mechanism due to Schallamach in Britain, Bartenev in Russia and Savkoor in Holland; and an engineering approach by Bulgin in Britain and Kummer in the United States. The present book, which is intended for engineers, represents essentially the engineering approach to rubber friction. The author has been much influenced by Dr Kummer and has indeed dedicated the book to his memory. Professor Moore does give weight to the atomic approach but a physicist would prefer to see this given greater emphasis. There is, for example, no discussion of the atomic model of rubber elasticity. There is an excellent chapter on viscoelasticity but it is all in terms of spring-and-dashpot models: there is no serious consideration of the molecular processes by which viscoelastic energy is dissipated, and no explanation of the physical meaning of a thermally activated process. Engineering students may, as a result, find it a little difficult to understand the atomic models of friction described in a later chapter since all of these, in one form or another, are essentially an extension of the Eyring rate-theory.

The author does not emphasize sufficiently the possible role of frictional heating on the viscoelastic process itself. There are good engineering models which provide a fairly reliable means of calculating the temperature generated (a) at the interface by the adhesion component of friction and (b) below the surface by the deformation component of friction. It is important to know to what extent such thermal effects, particularly at engineering speeds, modify the assumptions concerning the relation between viscoelastic properties and friction and wear. In the section on wear and wear mechanisms there is no reference to Schallamach's fascinating correlation between wear and viscoelasticity in the skidding of automobile tyres. Finally the author commits a rather intriguing solecism by translating a Gedankenmodell due to Prandtl as a "thinking" model—it was Prandtl who was doing the thinking, not the model.

Having expressed these comments it is only right for me to add that Professor Moore has produced a very systematic and timely survey of an area in tribology of great practical importance. The author clearly has a teaching course in mind, the book being aimed at third year engineering students or research men. He stands no nonsense, states his assumptions clearly, and proceeds to

work out specific models. Research men might prefer less arithmetic and a more critical assessment of the models and the assumptions. But students who wish to acquire a useful down-to-earth background to tribological aspects of elastomers will find it an extremely useful and informative introduction, although there are some confusing errors in the figures and text that need correcting. Apart from these defects, the book should prove of great value to teachers and students interested in the field in general and in particular to those concerned with the action of seals and the performance of automobile tyres.

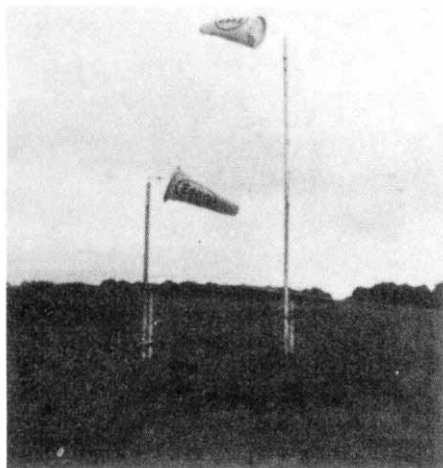
DAVID TABOR

Dye Fundamentals

Fundamentals of the Chemistry and Application of Dyes. By P. Rys and H. Zollinger. Pp. ix+196. (Wiley: New York, London and Sydney, August 1972.) £6.

WORLD dyestuff production runs at 560,000 tons per annum with a total value of 1,650 million US dollars. It is concentrated largely in five countries, the USA, Germany, Japan, Great Britain and Switzerland, which account for 55% of the production and 68% of the value. Thus dye chemistry is an important activity for a large number of organic and physical chemists. Surprisingly the organic chemistry of dyes, and even more so the physical chemistry of dye adsorption, receive scant attention in chemical education outside those few centres which make these topics their special concern, for example, at Leeds, Basle and Zürich. The authors of *Fundamentals of the Chemistry and Application of Dyes* believe that this springs from simultaneous growth of organic chemistry and the dyestuff industry over the past 120 years so that the latter is often felt by chemical purists to be relatively uninfluenced by modern chemical thinking. This view is quite wrong but there is some excuse for those outside the industry believing in it. Books on dye chemistry usually fail to reflect the movement of physical chemistry. On the one hand we have Venkataraman's monumental but uncritical review *The Chemistry of Synthetic Dyes*, while on the other there are general works in the old style such as Allen's *Colour Chemistry*. This new book by Rys and Zollinger based on their lectures at the ETH Zürich makes a welcome break with tradition. The main concern of the authors is with dye chemistry which is discussed throughout in physical-organic and mechanistic terms. The authors have adopted a simple and useful chemical classification of dyes which clarifies discussion. The main reactions of importance, for example, diazotization, are discussed in

Contrary Currents



Demonstration of an effect of lee waves with wind socks. Diagrams showing how this occurs accompany the photograph, which is reproduced in *Clouds of the World: A Complete Colour Encyclopaedia*, by Richard Scorer (David and Charles, Devon; and Lothian Publishing Co., Melbourne, 1972).

considerable depth. On the other hand dye application receives more cursory treatment. Of some seventy pages devoted to this topic, forty-six cover general technical matters. The physical chemistry of dye application is the subject of a short highly abstract treatment which provides an inadequate introduction to a very extensive literature.

By intensive cross reference the book covers a great deal of ground in relatively few pages and this makes for highly concentrated reading. It is thus a more economical buy than its price suggests. It is a valuable addition to the literature and it should achieve the authors' objective of arousing greater interest in dye chemistry. There are few errors of translation although I-acid for J-acid is an obvious slip.

I. D. RATTEE

Electrochemistry

Electro-chemical Science. By J. O'M. Bockris and D. M. Drazic. Pp. viii+300. (Taylor and Francis: London, September 1972.) £6.

THE resurgence of electrochemistry in the last twenty years has produced a vast number of texts on different aspects of electrode processes. Many of these have been of a very advanced and specialized nature, and the name of Professor Bockris—the senior author of the present book—has appeared on a substantial number of them either as author or editor. The book under review is a venture of quite a different type—an attempt to explain very

simply the fundamental ideas underlying processes at electrically charged interfaces, which the authors consider is the essence of electrochemical science, and to indicate the harvest of useful applications which may be expected to follow from its development.

This venture has succeeded magnificently, and the authors are to be congratulated on producing a book which is not only illuminating but exciting to read in its novelty and freshness. Some of their conclusions are quite unconventional. It is with some sense of shock and even a feeling of irreverence that one reads "As a result of Nernst's influence, electrochemical science in practice is half a century behind the progress it might possibly otherwise have made". However, the authors make out a good supporting case in the emphasis which Nernst's work gave to the thermodynamic treatment of electrochemical cells at equilibrium, whereas in practice a working electrode when electrolysis is occurring is rarely in this convenient state, and it is the kinetics of the processes occurring which are all-important. Throughout the book the ideas are developed always in the context of their present or future application in technology, and the variety and novelty of the applications considered are constantly entertaining. A very optimistic view is taken of an electrochemical future in which electrochemical science partners nuclear power in providing a world free from dependence on fossil fuels and with a cleaner environment. An example of how electrochemical science will alleviate pollution is the removal of discarded motor cars. They are to be dissolved in an ionic bath electrochemically, and the dissolved metals are then to be recovered separately by deposition with potentiostatic control of the cathode potential.

The main development of ideas in the book starts with a consideration of the structure of the electric double layer in an interphase, and why the potential difference across it can affect electrochemical reaction rates so greatly. This touches on many difficult fundamental points and clarifies them, and even experienced electrochemists can read it with advantage. The charge transfer across the layer is dealt with by the conventional energy barrier method leading to the concept of overpotential and its relation to current density; some indication of the quantum mechanical nature of electrode reactions is given. Transport processes and their great influence on electrode reactions in practice are dealt with in an admirably realistic way. These ideas are then used in the interpretation of the characteristics and problems of electrosynthesis, metal deposition, fuel cells, storage batteries,

corrosion, and electrobiochemistry. The book is well printed, indexed, and illustrated, and it should be regarded as essential reading for any student or teacher of electrochemistry.

A. HICKLING

Determining Defects

The Electronic Structure of Point Defects as Determined by Mössbauer Spectroscopy and Spin Resonance. By G. K. Wertheim, A. Hausmann and W. Sander. Pp. x+222. (North Holland: Amsterdam and London, 1971.) Hfl. 62; \$20.25.

THIS is a curiously disappointing book, certainly lacking the distinguished treatment so obvious in earlier volumes of the North Holland Series on "Defects in Crystalline Solids". Nevertheless, it contains a modicum of useful information. A basic fault is that the book hardly lives up to its title, since it is concerned mainly to identify defects rather than to discuss their electronic structure. Furthermore, the authors do not clearly differentiate between impurities and lattice defects, the treatment of the latter being scanty indeed.

Part I (Wertheim) is an extensive and readable account of Mössbauer studies: here the emphasis on impurities is greatest, perhaps understandably so. A very wide range of materials is discussed, as are such phenomena as order-disorder transformations and precipitation in various alloys. Part II (Hausmann and Sander) is devoted to the application of paramagnetic resonance methods to structural defects. The basic principles of electron spin resonance and electron nuclear double resonance (ENDOR) are outlined clearly and concisely. The level of treatment of the various types of solids investigated varies widely. Colour centres in the alkali halides are treated rather superficially. The authors go little further than 1962 in discussing colour centre spectroscopy in the alkaline earth oxides, despite a considerable body of research being published in the last decade. Rather more detailed treatment is given to the current situation in semiconducting crystals, although the heavy bias towards impurity effects is continued.

For the most part, the book is readily understandable, but it would be hard to obtain a full account of the existing experimental situation from reading it. The editors and authors would have been well advised to have used a somewhat different title and to have made an effort to produce a more integrated format. No doubt the cost allied to the rather dated information reviewed will result in this book being found mainly on the shelves of the wealthier reference libraries.

B. HENDERSON

Biological Sciences

Man in Homoeostasis

An Introduction to the Study of Man.
By J. Z. Young. Pp. xxv+719.
(Clarendon: Oxford; Oxford University: London, September 1971.) £6.

THIS is a monumental work. It ranges from the cellular make-up of the human body to the neurobiology of consciousness, from the information system of DNA to that of human speech and writing, from the origin of life to the origin of man, from the distribution of genes to the over-population of the Earth; it encompasses natural selection and social control; youth and senescence, conflict and cooperation, life and death.

Over this remarkable range the information is admirably organized and lucidly and vividly presented. The exposition proceeds logically from a consideration of molecular, physiochemical and genetic properties at the cellular level through the reproductive stages and the whole life cycle of the individual. From the individual it proceeds to an analysis of group biology, evolution of the species and the diversity of mankind. No other single work on human biology, probably no other single author, I believe, could provide equally satisfactory chapters on such topics as human evolution, human variation, the development of the brain, intelligence and speech, on bodily growth and ageing, and on consciousness. While much of the material presented is of necessity fairly standard, there are many new aspects and particular topics treated in Young's own inimitable, stimulating fashion. A good example is his chapter on the origin of life which develops into a discussion on the origin of self-maintaining systems—a topic of special significance in a book of this kind. As one might expect, he is especially illuminating when he deals with the many-sided theme of communication. It is hard to find a major topic which has been neglected, though I missed reference to the important work by Lieberman on the evolution of the vocal mechanism based on comparative studies of man and ape, which has made it possible to demonstrate that Neanderthal could not have had a language capacity at all comparable to modern *sapiens* man.

In a book so comprehensive and one which deals at every level with the notions of organization and integration, one may well ask to what extent this wide range of subject matter is given analytical coherence by the author. Professor Young achieves this by employing very effectively a number

Schwarzthal's Billy Goat



Living member of an ancient herd of goats whose home for at least five centuries was Bagot's Park in Staffordshire. The photograph is one of the illustrations to *The Wild Goats of Great Britain and Ireland*, by G. Kenneth Whitehead (David and Charles, Newton Abbot, 1972). £2.95.

of generalizing concepts or principles which appear variously as biological control, directed action, persistence, integrity, survival and pervasively as "homoeostasis". It is by virtue of the fundamental property of self-maintenance, exerted by the organism to counter stress, to satisfy needs or to ward off disease, through all the vicissitudes of development and of interaction with the environment, that he would wish us to understand the significance of the life activities of the individual, the local population and the species. He introduces the important notion of homoeostatic capacity. He sees bodily growth as a way of increasing, tissue repair a way of maintaining, and senescence the gradual loss, of this capacity. He

stretches the concept to include learning, intelligence and culture as devices for enlarging and improving the efficiency of homoeostatic response. The store of genetic variation available to a population and even random mutation are in the long run homoeostatic in that they ensure continuity, albeit with modification. He thus expands the use of the concept beyond the classical Bernard-Cannon doctrine, but as an organizing principle one must admit that it is heuristically convincing. Indeed, really to appreciate what the work is about, to get a clear "introduction" to this introduction, one can strongly recommend that the reader begins with the very last chapter in which Young provides "a summary of homoeostatic

reactions on various time scales".

Biology has much to offer for our understanding of the many problems which beset mankind and Professor Young takes pains to show that this is so. Yet Professor Young feels, in all humility, that as an introduction to the study of man (or perhaps more appropriately the "condition" of man) his book does not go far enough. He feels it is "unbalanced", even "superficial", because "it contains little about aesthetics, emotions, or spiritual life; nothing of humour, acting, music, or literature; nothing of architecture or invention, economics, sociology, or law; little of human joys and sorrows". But how much further can biological insight take us? It is not that Young wishes to promote a Wellsian, world-brain self-educator, but he feels that biology somehow should have more to offer for our understanding of these aspects of the "essence of life". He himself suggests tentatively that many social or cultural activities and institutions should be looked upon as manifestations of the biological urge to survival and to homeostasis.

This then is essentially a book of ideas and principles. Though it presupposes a reasonable general knowledge of basic science it is not a technical work since it is not concerned with the methods by which the functional or structural properties of the living body are observed or measured or populations investigated. Even so, it is very much the product of an experimentalist since Young is always critically concerned not to overstate the extent of our real knowledge of form and function. He constantly draws attention to problems crying out for investigation.

In its combination of biological erudition, concern for profound issues, scientific and social, sensitivity to the problems of mankind and not least to values and aspirations, this is a work of rare distinction. J. S. WEINER

Go to the Ant

Insect Societies. By Edward O. Wilson. Pp. x+584. (Belknap: Cambridge, Massachusetts. Published for Harvard University Press, August 1972.) £9.50.

In the literature of the late nineteenth century we can trace the emergence of a popular stereotype of the entomologist—as an elderly amateur with glasses and a butterfly net, apt to talk to himself and more or less mad (not always harmlessly). This figure has almost disappeared today, though traces of him linger, for example, in Nabokov. The professional entomologists of our century can hardly be said to have earned a recognizable literary image, though among the generality of their fellow scientists they probably have the repute

of being more than usually narrow-minded and petty. No one would expect an entomologist to win a Nobel prize, appear as a major scientist in the mass media, or reach the top positions in the Royal Society.

There are, however, entomologists today who can hardly be dismissed as petty or narrow-minded, and a prominent American example is provided by E. O. Wilson. His specialist work on ants has broadened to include almost every aspect of their life and evolution, and now culminates in a large work, the worthy successor of W. M. Wheeler's classic, dealing with the whole subject of social life in insects from the broadest evolutionary-biological point of view. The book is well illustrated, and written in a clear direct style, with such specialist technical terms as Wilson feels obliged to use explained in a glossary; evidently he is anxious to reach a wider audience than that provided by professional entomologists—as indeed the book deserves to do.

The author begins by summarizing current knowledge of each of the principal groups of social insects in turn, in 113 pages, pride of space naturally going to the ants, with forty-eight pages to only seventeen on termites. These accounts, compared with Wheeler's, include much significant information which was not known in 1928, and a good deal that has not yet found its way into the general literature. The extensive bibliography includes numerous works from 1969 and a few from 1970. The least satisfactory part of these chapters is probably his treatment of classification—presented in summary tables down to the genus level but with no characterizations of any of the taxa and no serious questioning of the bases of the systems (almost entirely from recent American works) presented. If and when the principles of phylogenetic classification come to be applied to his groups, we can expect considerable changes in the systems tabulated by Wilson. Following these chapters is one on "Pre-social Insects", with interesting examples drawn from spiders as well as from various insect orders. Curiously, the Embioptera receive only a single mention, and the Zoraptera none at all, in the entire book.

The remainder of the book is a series of chapters devoted to diverse biological topics related to social insects. A chapter on "The Population Dynamics of Colonies" draws fairly heavily on the work of the British M. V. Brian on *Myrmica* ants, and reproduces some of his mathematical treatment. Symbiosis, considered in two chapters, is taken to include all forms of parasitism, and a useful table of symbiotes of social insects is included. One of his more controversial chapters is one on "The Genetic Theory of Social Behaviour",

which would serve very well to stimulate student discussion groups, as would the following one on "Compromises and Optimization in Social Evolution".

Wilson devotes a final chapter to "The Prospects for a Unified Sociobiology", in which he states "The formulation of a theory of sociobiology constitutes, in my opinion, one of the great manageable problems of biology for the next twenty or thirty years". His comparison of vertebrate and insect societies leads to the challenging conclusion: "Had the vertebrates gone the same route [of reproductive specialization], the results would have been much more spectacular. The individual bird or mammal, possessing a brain vastly larger than that of an insect, might have been shaped into a comparably better specialist. The vertebrate society then could have evolved into an unimaginably more complex, efficient unit—at the price, of course, of independent action on the part of its members. Vertebrates have instead remained chained to the cycle of individual reproduction. This forever enhances freedom of the individual at the expense of efficiency on the part of society." He leaves us to draw what conclusions we will.

This handsome book will undoubtedly be widely read and influential; it should serve to recruit more students to the study of social insects, as well as to enhance the scientific status of entomology and of its author.

R. A. CROWSON

African Cichlidae

The Cichlid Fishes of the Great Lakes of Africa: Their Biology and Evolution. By G. Fryer and T. D. Iles. Pp. xv+641. (Oliver and Boyd: Edinburgh, May 1972.) £12.

Most authors must have been nagged at one time or another by the thought that some of their data would be "lost", or their impact obscured, through unavoidable publication in an inappropriate place. Those of us who have worked on African cichlid fishes can now relax, at least on that score. Fryer and Iles have done an extremely meticulous job of finely carding nearly half a century's literature. From the resulting strands they have woven a text which is likely to remain a first source of primary references to work on the biology and evolution of these fascinating fishes. New ideas and reappraisals of old controversies add to the book's value.

Rightly, I think, the authors have confined themselves to the cichlids of the major African lakes, especially Lakes Malawi, Victoria and Tanganyika. It is in these lakes that the cichlids have undergone their most spectacular outbursts of speciation and

adaptive radiation, an evolutionary explosion that surpasses in rapidity and extent that of the more widely publicized Galapagos finches or the Hawaiian flycatchers. However, it should not be forgotten that the South American Cichlidae (barely mentioned in this book, and then merely in a zoogeographical context) have played out, almost in counterpoint, an equally intriguing story.

Much of the basic research on cichlid evolution has stemmed from museum and field studies, but there is a body of experimental work which has important bearings on these other researches. A great merit of this book is the way in which its authors have brought together these different aspects, a synthesis that has never been attempted on such a comprehensive scale. Clearly a great deal more of such integrated work remains to be done, and hopefully this book will help to bring together workers in the various disciplines involved. The book's synthetic nature — in the best sense of that word — should at the very least introduce the Cichlidae to a wide circle of students and biologists.

Fryer and Iles attempt to review all aspects of cichlid biology and evolution, and to present them as a coherent whole. The greater part of the book (ten chapters) therefore provides background information on feeding and breeding habits, growth phenomena, synecological studies, genetics, and the morphological features which are basic to our interpretation of these life processes. Finally, all is brought together in three chapters outlining the evolutionary biology of the often perplexing species flocks that dominate the lakes. Other chapters treat the environmental background, the cichlids as a natural resource, and the subject of lake evolution itself.

As a specialist in cichlid fishes myself, I am perhaps the wrong person to judge how well (or badly) Fryer and Iles have presented their subject to a wider audience. I believe they have succeeded. Any author attempting to analyse and review such a vast subject has open to him two possibilities. Either he indulges in gross generalizations or he adopts a seemingly more ponderous, species by species account. Fryer and Iles have chosen the second method. To a non-specialist the names of numerous taxa may be baffling and a tax on his memory; but if he wants the facts and then the dessert of synthesis, he must work for them. Any other way would not be fair to the subject. The reader's task is somewhat lightened by the illustrations, but what a pity that no colour photographs of the true lake fishes are included (as they are in the American edition which, by the way, is cheaper too!). And why did someone choose as the cover illustration a cichlid (*Hemi-*

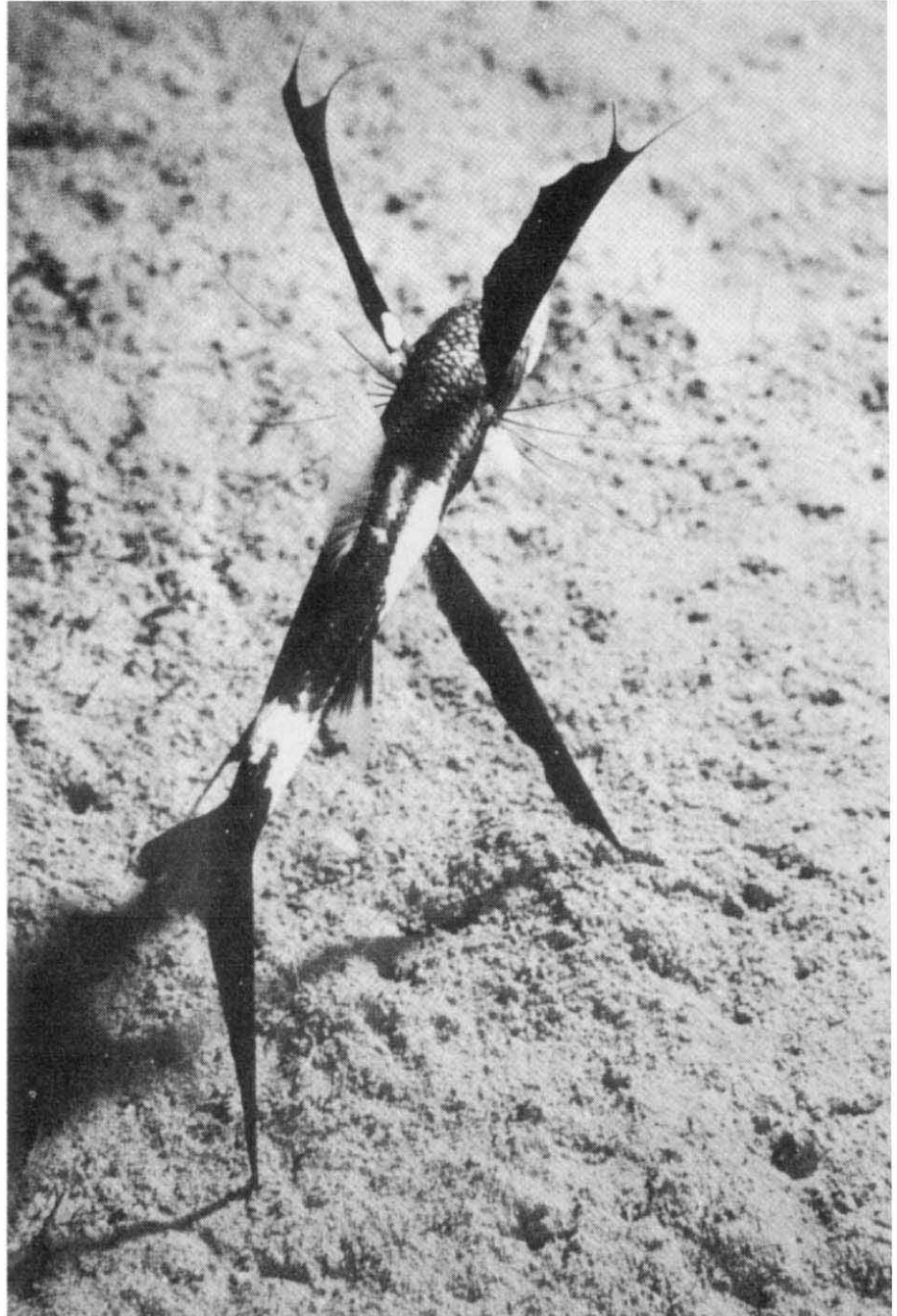
chromis bimaculatus) which, even with extreme zoogeographical generosity, can hardly be considered a "Great Lake" species?

In a notice of this length it is impossible to take issue with the authors on the several points where I disagree with their views or interpretations. Some of these disagreements may be almost on the level of family squabbles but there are others of a more general nature, and some should be mentioned.

First there is the section on structure and basic anatomy. This could do with an overhaul and is probably the weakest chapter in the whole book. For example, the figured jaw mechanism (supposedly typical) just does not work; the fish would dislocate its upper jaw every time the mouth was opened (a pity the authors ignored the highly relevant and elegant works of Osse and Liem on this subject).

Clearly Fryer and Iles are fascinated

Tripod Fish



Tripod fish, in ambush facing the current at the bottom of De Soto Canyon in the Gulf of Mexico: a fish which leaves footprints. One of the illustrations in *The Face of the Deep*, by Bruce C. Heezen and Charles D. Hollister (Oxford University Press, London, 1972).

by a recent theory of population density control through homeostatic mechanisms. But the examples they chose in support of this concept strike me as rather unconvincing. Most, if not all, could be simply direct reactions to purely external biotic factors. I find rather far fetched as an example of such a homeostatic mechanism the idea that females may actively jettison their broods, and particularly when it is linked with the evolution of those specialized predatory species that feed on embryos and yolk-sac-larvae. Jaw and dental specializations among the latter species suggest a more active role in getting their food than waiting for a supposed homeostatic mechanism to deliver it.

Finally, although I sympathize with the authors' dissatisfaction over the currently postulated interrelationships of many African cichlid genera, their revised phylogeny for the genera of Lake Tanganyika gives little satisfaction. It seems to be based more on intuition than on solid anatomical and other research; juggling with names and hunches is no substitute for these latter.

Doubtless every reader of Fryer and Iles's book will find some contentious points. That is how it should be, and in no way lessens the book's overall value as an outstanding contribution of use to specialist and general reader alike.

P. H. GREENWOOD

Whence Metazoa?

Evolution of the Metazoan Life Cycle: a Comprehensive Theory. By Gösta Jägersten. Pp. x+282. (Academic: New York and London, July 1972.) £5; \$15.50.

RECONSTRUCTION of the course of metazoan phylogeny has always depended heavily on embryological evidence. Haeckel's recapitulation theory and Garstang's neoteny did not prove to be the short cuts to understanding relationships between animals that more ardent supporters of each thought and it is now readily accepted that larvae, as much as adults, show adaptations to their environment. Purely developmental complications and the reproductive needs of the adults confuse the issue still further. Developmental and larval characters can provide valuable evidence of evolutionary pathways, but the task, with larval as with adult features, is to decide what has phylogenetic significance and what has not. Since we have a poorer understanding of larval ecology and adaptations than of those of the adults, such evidence as we have needs careful sifting and evaluation.

Professor Jägersten's book aims to do this. The present edition is a translation

of the original Swedish version (with a little tidying but no updating) published in 1968 and written two or three years before that. It develops from the author's bilaterogastraea theory published in 1955 and 1959, which is not explained here and with which the reader is supposed to be familiar. This theory postulates that the Metazoa are derived from a ciliated pelagic organism not unlike Haeckel's gastraea, which developed bilateral symmetry, three pairs of gastric pouches (which ultimately became coelomic compartments) and a gut with mouth and anus, and settled on the substratum.

It is contended that the life cycle of the early metazoans included alternating pelagic and benthic phases, the former eventually becoming the larval stages of benthic adults. Modern larvae (like the adults) have undergone much change and may include larval specializations, recent adult characters which have shifted back into the larva ("adulation"), and ancient adult characters, recapitulatory in the Haeckelian sense, as well as features which are archaic pelagic characters. More than three-quarters of the book is a systematic examination of the marine larvae of each phylum to decide which is which. This is followed by a section in which a number of general issues emerging from this analysis are taken up and discussed.

The systematic section assumes a basic knowledge of comparative embryology of the reader and contains much of interest as well as some new observations. It must be read, however, as an extension of the author's hypothesis about metazoan origins, and the interpretations that are offered are frankly partisan. This results in a certain amount of what seems to be circular argument and special pleading (the words "I feel that . . ." appear distressingly often in the text).

The bilaterogastraea theory demands that planktotrophic larvae are primitive, and evidence of direct development in supposedly primitive members of many phyla is dismissed without proper discussion. (Since support for the bilaterogastraea theory rests partly on the view that planktotrophic larvae are primitive, there is a circular argument here.) Lecithotrophic larvae are similarly dismissed without considering the ecological implications for the adults of producing small or large numbers of eggs. Atrochous larvae of polychaetes are claimed to be derived from forms with ciliary bands (which they may well be) without any justification for this view.

These are a few of the consequences of unqualified acceptance of the bilaterogastraea theory. It did not receive wide support when it was proposed, and so a large number of readers may find themselves dismayed at what seems cavalier dismissal of alternative

interpretations of developmental features, which, in other terms, are equally valid. The book would have gained in force and clarity if it had been more ambitious and expounded both the theory and the evidence for it together. The reader would then have been reassured that he was not presented with circular arguments and the author would have had an opportunity of evaluating the evidence much more rigorously than he has done.

R. B. CLARK

Useful Eukaryote

The Biochemistry and Physiology of Tetrahymena. By D. L. Hill. Pp. xii+230. (Academic: New York and London, 1972.) \$12.95.

Tetrahymena pyriformis, a small ciliate seen by many protozoologists in the last century, was the first free living protozoan to be isolated in bacteria free cultures. This was achieved by André Lwoff half a century ago. Although there has been constant interest in this organism ever since, it did not become a star until the 1940s, when it received its present name. *Tetrahymena* has been a favourite organism for the study of many, especially biochemical, aspects of protozoan life because it is one of the few eukaryotic forms which can be cultured in the absence of other organisms in undefined or defined media with yields and with the ease usually known only to microbiologists working with prokaryotic forms. Corliss¹ mentions that more than a thousand papers were published on *Tetrahymena* by 1965 and there are hundreds of new papers appearing every year. This vast amount of information makes *Tetrahymena* the most thoroughly studied free living protozoan. Our knowledge of it has been partially summarized in reviews but no book has been published about the organism, in curious contrast to the larger and formerly more popular *Paramecium* which can boast two monographic treatments^{2,3}.

The most important part of this gap is now beautifully filled by Dr Hill's volume, a clear and up to date critical summary of all important aspects of the biochemistry of *Tetrahymena*. It points out the basic similarities between this protozoan and other eukaryotic cells and reveals at the same time several peculiarities in its biochemical machinery which fascinate comparative and general biochemists alike (for example, the unique composition of the cell membranes containing tetrahymanol and phosphonolipids, glyconeogenesis via the glyoxalate cycle, thought to be a plant characteristic). The volume reflects the biochemical orientation of the author and it hardly

touches on the small amount of available information concerning the physiology of *Tetrahymena*. This is a minor criticism, however, made only because the title includes "physiology". This attractive and important book will be indispensable not only to protozoologists but to biochemists and biologists in general, providing ample food for comparative thought and stimulating much further experimentation.

MIKLÓS MÜLLER

¹ Corliss, J. O., *Acta Protozool.*, 3, 1 (1965).

² Kalmus, H., in *Paramecium das Pantoffeltierchen* (edit. by Fischer, G.) (Jena, 1931).

³ Wichterman, R., *The Biology of Paramecium* (Blakiston, New York and Toronto, 1953).

Life as a *Schein-Problem*

The Nature of Living Things: An Essay in Theoretical Biology. By Stephen Black. Pp. 156. (Martin Secker and Warburg; William Heinemann Medical Books: London, 1972.) £1.95.

The theme of *The Nature of Living Things*, as Stephen Black states it, is this: the second law of thermodynamics says that the disorder of the universe is increasing, yet it is obvious to common-sense that in the biosphere it is order, not disorder, that is increasing. Thus, he argues, there must be a conflict between evolution and thermodynamics. Where does the information come from?

The well-known answer (upon which Dr Black's essay is completely dependent) that Erwin Schrödinger gave to this perplexing problem was that an organism feeds on "negative entropy"; it gets its order from the environment. What is less well known, however, is that, in response to criticism by F. Simon, Schrödinger withdrew this formulation of the problem—pleading that he had been trying to explain highly technical ideas in simple terms and, for physicists, he would have used "free energy" instead. In later editions of *What is Life?* he also appended the following argument; geometrical orderliness is, in itself, unimportant because the entropy of diamond (which is as ordered as one could wish), together with that of the oxygen it burns in, is roughly the same as the entropy of the carbon dioxide produced, once the heat evolved has dissipated. Thus if there were an organism which fed on diamonds it would use the enthalpy change for its energy and it could solve the problem of entropy, not by doing anything as drastic as breaking the second law, but merely by being a little hotter than the surroundings, and dissipating the entropy, as before, as heat. This is not my argument, it is Schrödinger's. Unfortunately, while this clarification detracts little from Schrödinger's

book, it takes away the whole point from *The Nature of Living Things*.

The trouble with a word like "order" is that we have an intuitive idea of what it means (regularity perhaps, or tidiness), and it is surprising to discover that any evaluation of order is always relative to some external standard. In P. W. Bridgmann's words, from his *The Nature of Thermodynamics*, "All disorder is order from a different point of view". Thus, to illustrate this point, one might say that ABCD has a more "conventional" order than ABDC; *Botulinus* toxin has less "functional" order in men than it has in *Botulinus*; "schein-problem", to someone who cannot speak German, has less "linguistic" order than "apparent problem" and so on. But simply to equate all these totally different kinds of order with entropy is a (widespread) semantic confusion. When the second law states that order is decreasing it means that energy contained in one degree of freedom is continually getting spread over more degrees of freedom. In this sense what we could call "energetic" order is always decreasing, but functional order, linguistic order and any other sort of order can simultaneously do what they like.

Having stated this *schein-problem*, however, Dr Black attempts to solve it by declaring that DNA is Maxwell's demon. And he is quite right to highlight the problem of Maxwell's demon in biology. But not for the reason Dr Black supposes. If we accept the exorcism of this demon by Szilard then the real lesson, as Popper has shown (*Brit. J. Philos. Sci.*, 8, 151; 1057), is not that entropy and information are identical, but rather, as Pippard unequivocally states in his *Classical Thermodynamics*, that there is now no justification for the view that the second law is only statistically true. The correct conclusion to draw then, to use Popper's argument, is that we need a new statement of the second law which is consistent with Brownian motion and thus applicable at the molecular level. Thus the issues raised by (but not in) Dr Black's essay are by no means simple.

The answer to these problems, Dr Black suggests, is that "Life is a quality of matter which arises from mechanical realization of the informational potential inherent in the improbability of form". Now it is perfectly true that some problems are made much more simple if looked at in a new light, but, for me at least, this formulation is not helpful, because, using Sir Karl Popper's invaluable criterion, it is impossible to disagree with it. Life, apparently, is something to do with shape. Yes indeed, but what, exactly? After many pages of intrinsically interesting material and one very enjoyable poetic simile (a mutation is like a successful cadenza written into the score), Dr

Black claims that he has actually answered the problem. Unfortunately he has only restated it.

C. W. F. MCCLARE

Lampreys

The Biology of Lampreys. Edited by M. W. Hardisty and I. C. Potter. Vol. 2. Pp. xiv + 466. (Academic: London and New York, July 1972.) £6; \$18.50.

THE study of lamprey physiology and endocrinology has probably contributed more to our appreciation of the evolution of the sophisticated control mechanisms found in higher vertebrates than has the study of any other single group of animals. Consequently, Volume 2 of *The Biology of Lampreys*, with its emphasis upon physiology, will interest biologists of many different disciplines.

The first six chapters deal specifically with the endocrine system, yet the reader is left with many basic questions still unanswered. This reflects the state of our knowledge and not the competence of the authors. Perhaps the most valuable contribution is that of Larsen and Rothwell on the adeno-hypophysis. Many hitherto unpublished data on the ultrastructure and physiology of this gland are presented together with a good review of the previous work. But in the chapters by Sterba (Neuro and Gliasecretion), Eddy (The Pineal Complex), Barrington and Sage (The Endostyle and Thyroid Gland) and Hardisty (The Interrenal) it becomes clear that a high proportion of the available data is concerned with the structure of the endocrine system and as yet we only know a little about its functions. Barrington also discusses the phylogenetic implications of the unique structure of the pancreas, the absence of glucagon and the occurrence of secretin-like and pancreozymin-like activity in the intestine. This article, above all others, illustrates the important role of lamprey physiology in elucidating the evolutionary pathways leading to the situations found in higher vertebrates.

Lamprey haemoglobins and immunoglobulins are functionally similar to those of the gnathostomes, yet Riggs and Good, Finstad and Litman respectively demonstrate unique and peculiar properties of these blood proteins. Large differences in the circulatory system and respiratory capacities of larval and adult lampreys are considered in chapters by Fänge and Randall. These differences underline the caution needed in the analysis of presumed primitive conditions in a specialized animal. In a well-written chapter Morris depicts the possible evolutionary relationships between lampreys, hagfishes and teleosts when considered in terms of their

osmoregulatory mechanisms. Undoubtedly the most comprehensive chapter is one by Healey. More than 230 references, published throughout the past century, have been used to compile a full account of the central nervous system. In the discussion by Kleerekoper of the relative importance of the lamprey sensory systems the incomplete explanation of abbreviated labels in several diagrams proved to be distracting.

Unfortunately, a delay in the publication of Volume 2 has inevitably dated the book to an extent. However, the editors are to be congratulated for the wide scope, cohesion and absence of duplication throughout both volumes. This book will become a standard source of reference, yet at the same time should act as a stimulus to research

scientists to investigate many of the outstanding questions. One message that comes through most clearly is the call for more work on the predatory and marine phases of the animal's life history.

ALAN D. PICKERING

Phytogeography

Floristics and Paleofloristics of Asia and Eastern North America. Edited by Alan Graham. (Proceedings of Symposia for the Systematics Section, XI International Botanical Congress, Seattle, Washington, 1969, and the Japan-United States Cooperative Science Program, Corvallis, Oregon, 1969.) Pp. xii+278. (Elsevier: Amsterdam, London and New York, 1972.) Dfl. 75; \$23.50.

THIS is a collection of fourteen papers

contributed to the two symposia mentioned above, arranged as chapters.

As with many publications of this kind the items included vary considerably both in length and substance and not all of them can claim great originality, but among them are several important contributions to their subjects, and the book is therefore a worthwhile enterprise providing a useful source of information on a number of topics.

To the phanerogamist the most noteworthy chapter is certainly that by Yurtsev on "The Phytogeography of Northeastern Asia and the Problem of Transberingian Floristic Interrelations". This is a long and valuable account of the plant life east of the River Lena and north of 60 degrees N, a huge region about which many western botanists have had little opportunity of learning much and of which a great deal is likely to be heard in the future, and it is made all the more useful by a good bibliography including many references to the work of Russian botanists.

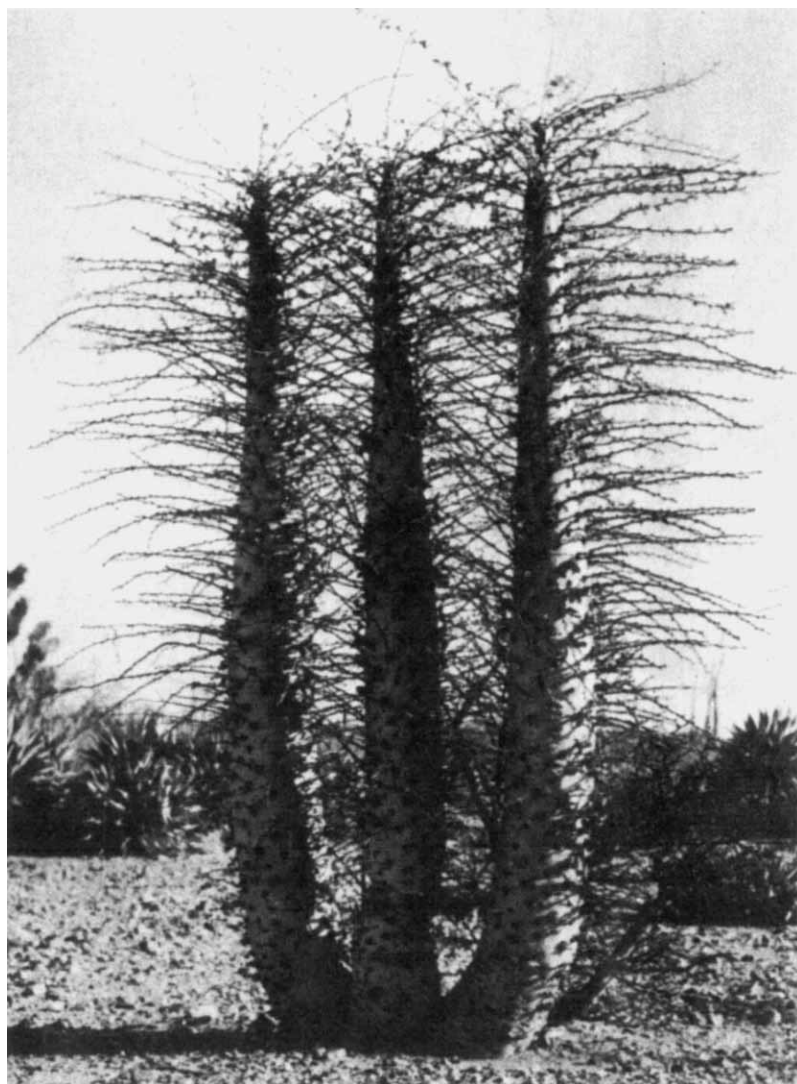
Palaeobotanists are also well served, by three chapters, the longest by Leopold and MacGinitie on Tertiary floras of the Rocky Mountains; by Wolfe, on the interpretation of Alaskan Tertiary floras; and by Tanai, who discusses the Tertiary history of vegetation in Japan.

For the cryptogamists there is a chapter, by Nishida, outlining the distribution of Japanese ferns; and another, by Kurokawa, concerning the lichens of Japan and North America. A long account of the bryophytes common to Japan and the United States, by Iwatsuki, is somewhat marred by the use of a Mercator-like projection for some of the maps, making one wonder when, at long last, botanists will cease to depict distributions outside the tropical regions in this way.

The remaining chapters are on subjects familiar to those interested in the distribution of the Angiosperms, and, although they should not be neglected, they do not break much fresh ground. The book is well produced and the translations from Russian and Japanese read easily.

R. GOOD

Boojum Tree



A member of the Fouquieriaceae, *Idria columnaris*, otherwise known as the Boojum tree. The thick stems are adapted to enable the plant to store water and survive in the desert of southeastern California. This is one of the photographs in *The Arena of Life*, by Lorus and Margery Milne (George Allen and Unwin, London, 1972; £5.25), a lavishly illustrated account of some aspects of plant and animal ecology in various parts of the world.

Ant Culture

Gardening Ants, The Attines. By Neal A. Weber. Pp. xvii+146. (The American Philosophical Society: Philadelphia, 1972.) \$8.

ANTS of genus *Atta* are perhaps better known as "leaf-cutting" or "parasol" ants because they carry pieces of leaf cut from a wide variety of plants into their nests. Many *Attines* are less conspicuous, but all use plant material to cultivate fungi in their nests and, although specific nest fungi are found with some other ants, only the *Attines*

are known to use this as the main source of food.

Weber's book is a detailed review of Attine biology with good illustrations and it deals with a full range of topics including a key to genera. His account of the cultivated fungi is particularly interesting although they have been difficult to name because fruiting bodies are not normally produced even when grown in cultures away from ants. Each ant garden is a pure culture of one fungus species even though different ant species have different fungi and contaminant spores must be abundant on the leaves when first cut. Weber considers that the culture methods used by the ants including salivary and anal deposits favour the cultivated fungus and "weeding" is of much less importance, but the ants clean themselves and the leaf pieces meticulously so contaminant fungi are probably more rarely introduced than one would expect.

To the ecologist the food chain leading to *Atta* is interesting because, compared with the usual predatory or honeydew feeding ants, a fungus has been substituted for a herbivore. Under the controlled conditions of the ant nests it may be a more efficient food chain and it is certainly effective—ten large *Atta* colonies are said to be equivalent to one cow and some South American governments treat them as a plague. Their excavations and recycling of nutrients, however, are likely to be important for stability of the ecological communities where they are abundant.

Although this may at first sight appear to be a very narrow specialist field, this book should be of general interest and it contains information of relevance to a surprisingly wide range of biological research. A. J. PONTIN

Collected Viruses

Virus Morphology. By C. R. Madeley. Pp. vii + 179 + 77 plates. (Churchill Livingstone: Edinburgh and London, 1972.) £2.50.

THE dimensions of most virus particles make them very amenable to examination by electron microscopy, but the published results do not occur in a form convenient for comparisons to be made. They are distributed over a large number of journals and the considerable variations in the techniques used for microscopy can themselves lead to differences in appearance. The collection of electron micrographs in this book, obtained from specimens prepared in the same way—negatively stained with the same stain—and reproduced to the same scale, thus provides a useful series of immediately comparable images. The book is aimed at students of medical virology and so

the seventy-odd viruses shown are from those infecting man, but their structures are typical examples of viruses in general, although there are no counterparts of the rod-shaped and head-and-tail plant and bacterial viruses.

The author provides a glossary of common structural terms although these are sometimes confused in the text—it is difficult, for example, to imagine how a morphological subunit on the surface of a virus can appear as a hole. The presence of symmetry could also be clarified if it were pointed out as being a consequence of the subunit structure rather than the reverse. The unique collection of electron micrographs is, however, the main feature and makes it a useful reference book. J. T. FINCH

Alcoholic Biochemistry

The Biology of Alcoholism. By Benjamin Kissin and Henri Begleiter. Vol. 1: Biochemistry. Pp. xxviii + 630. (Plenum: New York and London, 1971.) \$36.40.

INFORMATION about many aspects of the biology of alcoholism is available in specialized articles and in reviews. Collections of articles by various authors can, by appropriate and wise selection of titles for the chapters, integrate existing facts and new developments in the subject into a valuable reference volume for the student and research scientist. When, however, some of the contributors to such a book produce reviews on the specific subject selected for them, and others write about their subjects primarily from the aspect of their own field of research, the final product tends to be unbalanced and repetitive. This criticism can be applied to volume 1 on biochemistry and to volume 2 on physiology and behaviour in relation to the *Biology of Alcoholism* by Kissin and Begleiter. The different chapters reflect the characteristics of the individual contributors, often presenting good reviews only of their special fields of research. There is little cross reference between the chapters.

The first chapter in volume 1 by Dr Kalant covers the absorption, distribution, and elimination of alcohol together with a section on its effects on biological membranes in which ion movements and transport phenomena are discussed. In his discussion, similar references are quoted and the same general conclusions are reached as those mentioned by Dr Wallgren in his review on intracerebral respiration and brain function. Dr Von Wartberg presents an up-to-date account of his work on the oxidation of ethanol to acetaldehyde by alcohol dehydrogenase, and then to acetate or its activated form by aldehyde

dehydrogenase. In most species, he writes, total *in vivo* oxidation of acetaldehyde is faster than that of alcohol, and there is general agreement that ethanol oxidation by liver alcohol dehydrogenase represents the rate-limiting factor for alcohol removal from the whole organism. In contrast, when one reads the statement by Vitale and Coffey in their discussion of alcohol and vitamin metabolism, "it appears that the principal site of alcohol oxidation is the liver . . . it seems likely that the liver is the limiting factor in these studies upon alcohol oxidation", and their supporting evidence, it becomes apparent that volume 1 is a series of unconnected, but interesting and individually stimulating review articles. In volume 1, Truitt and Walshe present another description on the role of acetaldehyde in the actions of ethanol. They discuss the action of serotonin, just as does Feldstein when describing the effect of ethanol on neurohumoral amine metabolism, and Myers and Veale when discussing the determinants of alcohol preference in animals in volume 2. It is an effort to identify these different aspects of the same subject from the index, and when one has identified them, one frequently finds the same references quoted and the same conclusions reached in different parts of the two volumes.

The first chapter of volume 2, on the effects of alcohol on the neurone, gives an account of the relationship of sodium and potassium ions, and of Mg^{2+} and Ca^{2+} , to ATPase. This is comparable to the accounts in volume 1 which quote similar references; however, the conclusions about the effects of alcohol on membrane permeability are not comparable. The succeeding chapter gives an interesting and valuable review of peripheral nerve and muscle disorders associated with alcoholism. Subsequent reviews deal with consumption of alcohol by different animal species, and the genetic, physiological, environmental and behavioural variables which influence it. In their review, Myers and Veale draw attention to the importance of the element of choice, which should be provided for the animal between the alcohol test solutions in the experimental design, in all investigations of alcohol preference in animals, particularly those including genetic or behavioural factors. This can greatly influence the interpretation of experimental results.

The account of conditioned aversion and behaviour modification for the control of excessive drinking by Vogel-Sprott in volume 2 is concise and clear. It compares very favourably with the descriptions of the alcohol-disulfiram syndrome which are scattered through the two volumes. The clinical features of this, and similar reactions produced

by other agents, are referred to as a *sensitization* even though the biochemical basis of the reaction is explained by Truitt and Walshe in volume 1. This is unfortunate because true allergic hypersensitivity to alcoholic beverages is a known clinical condition, and no reference to this syndrome appears in either volume. Numerous scattered references appear to the actions of drugs. As an example, Von Wartberg states that ambiguous results have been reported from clinical studies with metronidazole and tells the reader to see volume 1. However, there are no further references to this drug in the index of either volume. None of the chapters in either volume deals exclusively with the relationship between alcohol and drug action.

Volumes 1 and 2 of the *Biology of Alcoholism* contain much worthwhile accumulated information in a series of review articles in which the facts and references may be repeated on several occasions. As a book, however, it lacks the integration and cohesion to make it into a comprehensive reference manual. The individual articles provide good reviews of aspects of the biology of alcoholism but there are gaps in the subject matter. The indexes are insufficiently detailed to enable the reader easily to select the relevant facts about the subjects in which he is interested. These volumes provide an expensive method of obtaining a series of good review articles by experts on their subjects between four covers. They will provide worthwhile additions to reference libraries. C. W. M. WILSON

Mutation Research

Chemical Mutagenesis in Mammals and Man. Edited by F. Vogel and G. Rohrborn. Pp. xiv+519. (Springer: Berlin and New York, 1970.) 124 DM; \$34.10.

THIS book contains valuable chapters on mutation research contributed at a symposium held in Maine in 1969, and several additional chapters thereafter. In the preface, the editors provide an overall masterly introduction to the material covered in the book. The first three chapters deal with molecular mechanisms involved in the production of spontaneous or induced mutations and also on the potential impact of chemical mutagens in the human environment. The following fourteen chapters describe research methodologies, such as dominant lethal test, *in vivo* cytogenetic studies and host-mediated assay for practical screening of potential chemical mutagens. Systems for detecting the induction of recessive mutations and chromosome aberrations in mammalian somatic cell cultures, including man, are also dealt

with. Even though the main aim of this book is to emphasize methodologies for those who are concerned with the problem of chemical mutagenesis in relation to man, various results obtained to date with available methods on some chemical groups, such as alkylating agents, antimetabolites, acridines and purines, are described in the next ten chapters. All in all, this book is an important addition toward the understanding of many problems relating to chemical mutagenesis.

SAMUEL S. EPSTEIN

Vertebrate Physiology

How Animals Work. By Knut Schmidt-Nielsen. Pp. vi+114. (Cambridge University: London, July 1972.) £2.20; \$5.95.

BASED on the Hans Gadow Lectures for 1970-71 at the University of Cambridge, Professor Schmidt-Nielsen's latest book is aimed chiefly at undergraduate students of vertebrate physiology: it merits a far wider audience. Indeed, it is a pleasure to find quite complex concepts explained with such beguiling simplicity and lucidity. In spite of its broad title, this little volume is directly concerned with those aspects of vertebrate physiology to which the author and his colleagues have made such valuable contributions during the last two decades or so, especially in relation to desert adaptation. It is thus an expression of Knut Schmidt-Nielsen's personal views; but these are conveyed with such conviction that even his sternest critics must be swayed by the power and reason of his arguments.

This incisive account first illustrates how the principle of countercurrents has been invoked to reduce water-loss through evaporation in the respiration of kangaroo-rats. A temperature gradient is established and heat exchange results in the temperature of the exhaled air approaching that of the ambient air (according to the principles of the Stirling and Carnot heat engines). The legs of wading birds and the flippers of whales are next cited as further examples of countercurrent heat exchange. The problem of losing heat by panting is now discussed, and attention turned to the respiration of birds. New evidence from the author's laboratory suggests that air flows unidirectionally through the bird lung, against a countercurrent of blood. (A similar system occurs in the fish gill, but is not possible in the lungs of mammals.) This mechanism enables birds to migrate for great distances without excessive water-loss, and to fly at altitudes so great that mammals would barely be able to survive in them. Since respiration is synchronized with wing beat, tidal volume in trumpeter swans must

be adjusted to a low frequency. A large dead space is therefore necessary to achieve a suitable concentration of carbon dioxide in the posterior air-sacs which comprise the mixing chambers, and this is provided by the elongated trachea.

The expenditure of energy in locomotion is next discussed and the need for animals to use evaporation of water for keeping cool. The principle of countercurrents in the carotid rete enables their heads to remain cool, even though the bodies of gazelles and antelopes may reach temperatures over 46° C. Independent control of temperature by the same means in limited parts of the body is responsible for the high muscle temperatures of tuna fishes and sharks. Such countercurrent systems utilize vascular channels of the millimetre size range, which means that they cannot inadvertently function as gas exchangers.

The book next compares such exchangers with multipliers like the swim bladders of fishes (which can secrete against pressures of several hundred atmospheres) and the mammalian kidney. It concludes with a discussion of problems of scaling. Schmidt-Nielsen clearly vindicates the opinions of those zoologists who believe that university teaching ought to be original and to provide exciting and stimulating ideas. More is needed than merely to provide a *ésumé* of facts culled from the textbooks and reviews currently fashionable. Carl Pantin would surely have approved of Schmidt-Nielsen's lectures.

J. L. CLOUDSLEY-THOMPSON

Discriminating Enzymes

Stereochemistry and its Application in Biochemistry: The Relation Between Substrate Symmetry and Biological Stereospecificity. By William L. Alworth. Pp. xi+311. (Wiley: New York and London, August 1972.) £7.75.

THIS book sets out to show, to any student or practitioner of biochemistry who can lay hands on a set of molecular models, why a reaction proceeding on an enzyme can often discriminate absolutely between two groups (in the same molecule) which would be attacked with precisely the same probability by ordinary, symmetrical chemical reagents. The aim has been achieved: anyone prepared to study the text with attention and a modicum of intellectual effort will be able to grasp the author's explanations and to apply them. Indeed, there may be a different risk: an intending reader might think that a subject requiring so much explanation must be intrinsically difficult. To him, I would say: "Don't be afraid; all this discrimination and specificity of enzymes arises simply from their power (just like yours with molecular models) to distinguish

between chemically different groups and between different arrangements of groups in three-dimensional space. If Dr Alworth's explanations seem elaborate, it's only because he doesn't want to give you any chance of getting the subject slightly wrong, as he thinks many biochemists do. He is lucid as well as careful."

The reader is taken through an historical introduction, and discussions of symmetry and of molecular dissymmetry. The terms and conventions of stereochemistry are carefully explained, though it could be objected that more detailed discussion is given to the obsolescent D, L system than to the rational R, S system which is superseding it. Methods of assigning absolute configurations are described. The ground having been cleared, the stereo-

chemical basis for distinguishing between "paired groups" in a molecule is considered in depth. Enzyme-substrate interactions are examined in the particular case of α -chymotrypsin. There is a long final chapter describing the use of stereochemical methods to investigate the mechanisms of a variety of enzymic reactions.

I have already said that the author has succeeded in his aim; but one could have wished that the stereochemical specificity of enzymes had been shown more clearly as the necessary consequence of their substrate specificity and of their role as catalysts. It could also have been brought out that our ability to demonstrate an enzyme's discrimination between "paired groups" depends absolutely on our power to put a private mark on one of them, by means of an

isotope. Without isotopes, a logician might eventually have inferred this property of enzymes; but his only chance of proving it would have been to inspect somehow, at the molecular level, a substrate reacting at an enzyme's active site. As it was, Ogston's note in this journal in 1948, which laid the foundation for the whole subject, was an explanation of results already obtained with isotopes.

Blemishes in the text are agreeably few, but errors were noted in several diagrams. J. W. CORNFORTH

Evolutionary Essays

On Evolution. By John Maynard Smith. Pp. viii + 125. (Edinburgh University: Edinburgh; Aldine Atherton: Chicago, 1972.) Paper 75p; hardback £1.50.

EVOLUTIONISTS often claim that their science includes in its domain all of biology, that the study of the history and dynamics of change of organic forms is the study of biology in its most complete sense. Despite this catholicity of professed interest, "evolutionists" turn out, on examination of their professional work, to be comparative anatomists or palaeontologists, or statisticians, or population geneticists, or biochemists, or something else rather specialized. Some few have made forays into philosophy or history of biology, often as amateurs and dilettantes. But never has anyone made professional and non-trivial contributions to such a diversity of fields as, say, population genetics, functional anatomy, developmental genetics, insect physiology, optimality theory and the philosophy of science, all in an evolutionary context. Never? Well, hardly ever. For there is John Maynard Smith, whose new collection of papers and essays, *On Evolution*, reminds us that it is possible to be an "evolutionist" after all. What is required is an extraordinary intelligence, a high degree of originality and imagination, a bird-watching boyhood in Dartmoor, a training in dialectical thinking, professional experience as an aircraft engineer, and many years of close association with another polymath of evolutionary biology like J. B. S. Haldane.

On Evolution is a sample, and by no means an exhaustive one, of the various evolutionary problems to which Maynard Smith has contributed solutions. The papers are not technical, but are not condescending in their simplicity either. They can all be read profitably by any natural scientist or student of natural science, and most of them are well within the capability of any serious intellectual. The topics include the evolution of animal flight, the develop-

Developing Nerve



A nerve fibre during development of the myelin sheath. Microtubules are visible in the Schwann cell cytoplasm (X). Photomicrograph from Hirano's chapter on "The pathology of the central myelinated axon" in *The Structure and Function of Nervous Tissue*, volume V, *Structure III and Physiology III*, edited by Geoffrey H. Bourne (Academic Press, London and New York, 1972).

mental genetics of pattern formation, eugenics, the origin and maintenance of polymorphism, the evolution of sex and three historico-philosophical articles on evolution, time and history. For me, the most exciting chapter is the first, a previously published essay, in which Maynard Smith introduces a wholly new concept in evolutionary theory, the *evolutionarily stable strategy*. He shows how the apparatus of game theory, which previously appeared to be useless in evolutionary studies except as a metaphor, can now be made to generate important predictions by use of this new concept. The applications of this new theoretical advance are certain to be much wider than the behavioural problem discussed by Maynard Smith in his essay.

On Evolution is the only collection of scientific essays I have ever read that made me stop on page after page and say to myself "What an interesting idea. I never thought of it that way before". I wish I had written it myself.

R. C. LEWONTIN

Electron Transfer

Electron and Coupled Energy Transfer in Biological Systems. Edited by Tsao E. King and Martin Klingenberg. Vol. 1. Part A. Pp. xiii+324. Part B. Pp. x+325-729. (Marcel Dekker: New York, 1971.) Part A, \$19.75; Part B, \$28.50.

ONE of the central processes of biological systems is the mechanism of energy transduction. Of general interest in this respect is the way in which the free energy liberated by electron transfer reactions (oxido reduction chain) is transformed into "energy rich" covalent chemical bonds.

This book intends to give a treatise of electron transfer processes and coupled energy transduction in biological systems. It contains twelve selected topics, each written by known specialists actively working in the field of electron transfer and oxidative phosphorylation. The book is therefore a collection of twelve different reviews for special problems which do not provide a general insight into the fundamental problems of the energy transducing mechanism. It is mainly restricted to mitochondrial processes (except for three chapters), so that the title of the book seems to be not correct.

Chapter 1 (by R. Lumry) reviews the physicochemical (especially thermodynamic) properties of globular proteins. It provides a good insight into the complexity of the factors which govern protein structure and function and their interrelationships. After this the reader would expect in the next chapter the application of these general considerations on the special case of

energy transducing systems, their structural and functional properties within the membranes of biological systems. In the same way the reader will be disappointed by the lack of physicochemical considerations concerning the fundamental process of energy transduction (thermodynamics and kinetic aspects, models for the energy coupling mechanism).

The book presents in chapter 2 a short review (by Wang) of chemical model systems in homogeneous solution. On the basis of these results a model for phosphorylation *in vivo* is proposed in terms of the chemical hypothesis.

In chapters 3-6 and 8 the nature of some mitochondrial enzyme complexes is discussed (DPNH-dehydrogenases by Huennekens and Makler; NADH-ubiquinonereductase by Sanadi *et al.*; choline and α -glycerophosphate dehydrogenase and electron transfer flavoproteins by Frisell and Cronin; the b-group cytochromes by Sekuzu; and by Wilson and Dutton the redox-potential changes which are connected with the mitochondrial energization, the cytochrome oxidase system by Sekuzu and Takemori).

In chapter 7 (by Kamen *et al.*) the properties and the functional role of cytochromes in biological systems are described; chapter 10 (by Crane and Sun) summarizes the role of lipids in mitochondria (a small part includes other systems like autotrophic bacteria and chloroplasts). Special oxidative phosphorylation systems are reviewed in chapters 11 and 12 (invertebrates by Tappel, microbial systems by Brodie and Gutnick). A more general description of the physical and chemical properties of metalloprotein-enzyme systems (excluding haem proteins) is given by Palmer and Brintzinger in chapter 9. This review provides a good insight into the powerful tool of spectroscopic methods.

The book seems to be more valuable for researchers who want to become familiar with these special topics than for readers who are interested in a more general point of view about the central problems of energy transducing systems.

Essential points of these mechanisms are not discussed. The very important phenomena of ion transport and electrical field generation which are necessarily coupled to each anisotropic electron transfer in membraneous systems are not reviewed; the current hypotheses about the coupling of electron transport systems with the energy transducing enzymes are not presented.

Because of these facts the book does not provide a complete picture of our present knowledge about the subjects discussed, but gives only partial and selected topics.

G. RINGER

Biochemical Spectra

Biochemical Applications of Mass Spectrometry. Edited by George R. Waller. Pp. xiv+872. (Wiley: New York and London, June 1972.) £19.75.

PROFESSOR WALLER has edited a remarkable book. As the title implies, the bulk of this work is concerned with the biochemical applications of mass spectrometry. These are included in section III and comprise chapters on fatty acids; complex lipids; steroids; bile acids; carbohydrates; terpenes and terpenoids; amino-acids; amino-acid sequence in oligopeptides; nucleic acids and derivatives; antibiotics; vitamins and cofactors; hormones; drug metabolism; tetrapyrroles; clinical use of mass spectrometry; pesticides; alkaloids; flavour compounds, and seriochemicals. There are also articles on the use of stable isotopes, negative ions, mass spectrometry in the detection of life on other planets, determination of the structures of organic molecules and qualitative analysis with field ionization mass spectrometers, and chemical ionization mass spectrometry. Other sections cover instrumentation and the interpretation of mass spectra. All the articles in this book are written by experts in the appropriate fields. Each subject is accompanied by an adequate bibliography and the whole work is completed by a subject index. There is an introduction as well as two essays, one of which is an appraisal of the work of Professor Rittenberg.

All sections are of a high standard and, moreover, they often represent a relatively new assessment of a chosen subject. This is noticeably true of the contributions on seriochemicals which extend the range of the mass spectrometer, and on the automation of scientific inference which seeks a routine method for structure determination based largely on the mass spectrum, surely one of the most urgent and rewarding of problems.

This book represents a comprehensive survey of the available information and as such is an invaluable reference work for researchers in this and cognate fields. It will also prove essential reading for anyone wishing to enter mass spectrometry.

It is remarkably free from misprints and those I noticed are mainly trivial and should not confuse the careful reader. The most serious is probably the formula of retinal (page 501) where a ring methyl group has been omitted.

The publishers have produced a very pleasing book which is clearly printed with good and profuse illustrations. It represents a major work of scholarship. The publishers, the editor and the contributors are to be congratulated on their effort, for this work must surely become a standard reference for many years.

R. I. REED

Lab. Psychopathology

Experimental Psychopathology: Recent Research and Theory. Edited by H. D. Kimmel. Pp. xiii+264. (Academic: New York and London, December 1971.) \$12.50; £5.85.

FOR the greater part of its sixty-year history, experimental psychopathology, restricted here to studies of disorders of behaviour induced by deliberate manipulation of independent variables, has exerted little influence on psychiatry although it has contributed to the progressive shift of emphasis from genetical, biochemical and neurological explanations to explanations in terms of psychological processes. More recently its shortcomings in concept and method have been overcome in some degree, and this review of current research is timely, especially since it is introduced by re-evaluations by Horsley Gantt and Jules Masserman of their pioneer work.

Pavlov regarded disorders as the accidental products of a brain whose functions had broken down through "overstrain" of excitatory or inhibitory processes, and did not try to explain them through analysis of the "difficult" conditions in which they had been shown to occur. The editor, restating Masserman's "uncertainty principle", points out that there are always two stages, the learning of adaptive patterns, and their confrontation with altered environmental demands which are uncertain, unpredictable or capricious. Disorders are the maladaptive consequences of adaptive behaviour. The question: "What is the special nature of the difficulties?" awaits a full answer.

The mixtures of symptoms which make up Pavlov's experimental neurosis have proved too complicated for study, and current researches focus on single aspects. Amsel, for instance, discusses the persistence, and Sandler and Davidson the extinction, of behaviour which is frustrated, that is, not rewarded when reward is anticipated. Persistence despite "pain" is the essence of Freud's neurotic paradox. The somatic concomitants of disorders of behaviour have assumed great importance, and Joseph Brady, known for his production of gastro-intestinal ulcers in "executive" monkeys, reviews, with Jack Findley, work on cardiovascular disorders, especially hypertension. The book ends with an account by Harry Harlow and his collaborators of the responses of infant rhesus monkeys to cloth and wire surrogate mothers.

One cause of its lack of influence on psychiatry is that experimental psychopathology has continued in the tradition of the psychological laboratory of studying the reactions of individuals to discrete environmental events. Psychiatry, on the other hand, has become increasingly concerned with social behaviour

made up by sequences of interactions between persons. Harlow has prepared the way for the researches now being carried forward, perhaps notably by Robert Hinde in Cambridge, in which the surrogates have been replaced by natural mothers, and disordered patterns of interaction are systematically studied. The gap is being narrowed, but to intend a book for both scientists and clinicians seems unduly optimistic.

D. RUSSELL DAVIS

Fixing Nitrogen

Biological Nitrogen Fixation. By J. Postgate. Pp. vi+61. (Marrow: Watford, 1972.) £1.25; \$4.

THIS small monograph by Dr Postgate is well written, concise and is easily read. He covers in a fairly complete way all of the current aspects of the fascinating problem of dinitrogen fixation. Anyone with a reasonable degree of biochemical knowledge should be able to read the book and follow it.

The book is organized in a logical manner. It uses tables and figures to good advantage. It starts out by describing the organisms or systems that fix dinitrogen and then goes into the most significant advances that have been made during the past ten years in the enzymology of the problem. Starting with the discovery of the first cell-free dinitrogen-fixing extracts, he leads one through the experiments that show the requirements for ATP, Mg and reduced ferredoxin and that demonstrate that two distinct protein components are needed. There is a discussion of the various substrates that are reduced by nitrogenase. The role of ATP and metal ions in N_2 fixation is discussed and he points out clearly that present evidence is scanty.

The section on the physiology of N_2 fixation discusses the role of oxygen, the nutritional requirements for organisms that fix N_2 , and the probable sources of electrons for N_2 reduction. This section is followed by sections concerned with the control and regulation, ecology, evolution and chemistry of N_2 fixation.

A few minor mistakes or omissions in references are made. The first demonstration that H_2 and ATP supported nitrogen fixation was not by the cited authors. Kennedy, Jeng and Mortenson was the reference for the demonstration of Mg^{2+} requirement for nitrogenase and not Moustafa and Mortenson. Also the first references showing that ATP was required (McNary and Burris, *J. Bacteriol.*, **84**, 598; 1962; and Mortenson, *Proc. US Nat. Acad. Sci.*, **52**; 272; 1964) are not included.

Postgate's prediction in the preface has come true. Some items in the text have changed or are now no longer true.

One example is the effect of CN^- on the Mössbauer spectrum of ^{57}Fe K α_2 . Further purification by that group has shown that the effect was the result of an impure preparation. But this does not significantly change the context of the book, since he stated that the results should be taken with caution.

In summary I feel this small book well worth reading. It is both informative and entertaining (from a scientific point of view). L. E. MORTENSON

Soil Fungi

Ecology of Soil Fungi. By D. M. Griffin. Pp. ix+193. (Chapman and Hall: London, 1972.) £2.80.

AT last a book has been published that can honestly be claimed a "must" for all who study soil ecology. The author, who never commits himself to paper unless he has something of significance to say, must be a delight to work with, albeit a stern taskmaster.

The book is in two parts. The first is a general study of factors such as the composition of the soil, fungal physiology and effects of other microorganisms, while the second discusses in detail the physical properties of the soil and their significance in the establishment and growth of soil fungi. A full mathematical treatment is given where possible.

The author is well aware of the difficulties of assessing quantitatively the complex interactions of the soil, soil microorganisms and the fungi. To do this with any degree of accuracy is extremely difficult. But the author points out pathways of research which can lead to the achievement of more precise results and therefore more significant theses. His descriptions of work done to assess the effects of various soil factors on the growth of fungi are always careful and precise. To me, the most important parts of this section are the warnings that laboratory conditions can never replicate exactly conditions in nature, and one must realize that what seems clearly established in the laboratory may not be so in the field. The author frequently demonstrates how detailed analyses have often uncovered remarkable differences between adjacent and superficially similar sites. I like the author's way of gently rebuking workers who have used assumptions as if they were established facts. One can be sure that when the author himself uses the word "probable", he is commenting from a wide range of research and experience, but needs more facts before he will say, "This is so".

Altogether this is a very useful and informative book. I look forward to another, when some of Dr Griffin's "probables" have been turned to certainties. MARGARET T. POTTS

(Continued from page 264)

$0.25 M_{\odot}$ the minimum luminosity is some 5% lower than the luminosity before mixing. At this point, although much of the interior is in a relatively expanded state the radius at the photosphere is somewhat smaller than normal. The cooling of the outer envelope which results from the smaller heat input from below allows the temperature and pressure to fall and the surface layers to contract. Later, the luminosity rises as the thermal wave produced by the burst of energy generated immediately after mixing reaches the surface, and finally the star settles down with a luminosity somewhat lower and an effective temperature slightly higher than it had before mixing began.

Finally, we consider the neutrino flux. The mixing of ${}^3\text{He}$ towards the centre initially increases the rate of ${}^7\text{Be}$ production and the ${}^7\text{Be}$ neutrino flux $F_{\nu 7}$ rises. But mixing of ${}^7\text{Be}$ away from the centre reduces the rate of the temperature sensitive proton capture and the ${}^8\text{B}$ neutrino flux $F_{\nu 8}$ drops rapidly. As the central temperature subsequently falls $F_{\nu 7}$ attains a maximum and then decreases, and $F_{\nu 8}$ drops still further; temperature is the dominant influence on the pep neutrino flux $F_{\nu p}$ which also decreases. The ${}^7\text{Be}$ and ${}^8\text{B}$ fluxes reach a minimum at the same time as the central temperature, and then gradually rise as the Sun relaxes back to thermal equilibrium. The precise details of the evolution depend on how much of the core is mixed; the model illustrated in Fig. 1 is typical. Because many of the recent improvements in the microphysics have not been included in the calculation, $F_{\nu 8}$ in Fig. 1 has been scaled by a constant factor to make its value prior to mixing similar to the latest published theoretical results. R. Rood and D. N. Schramm (private communication) report similar results. The minimum value of the total neutrino flux is of the same order as the currently quoted experimental upper bound. It appears, therefore, that if it can be demonstrated that the Sun has recently been mixed, the observed neutrino flux may no longer present a serious problem. The possibility still remains, however, that more refined measurements will lower the upper bound still further. No doubt a model can be found for which $F_{\nu 8}$ (which is very sensitive to comparatively small adjustments of the model) is negligible, but it is unlikely that the sum of $F_{\nu p}$ and $F_{\nu 7}$ can be reduced appreciably below about 0.7 s.n.u.

Solar Chronology and the Terrestrial Climate

The most recent of the Earth's major ice ages were separated by approximately 2.5×10^5 yr (ref. 37) which is of the same order as our rough estimate of the interval between successive mixing phases. The view that reductions in the solar luminosity L were responsible for the Earth's glaciations^{7,38,39} is not universally accepted³⁷, but this is chiefly because evidence for solar luminosity variations has not been considered convincing. If major ice ages are initiated by fluctuations in L they must be a transient response, for it is believed that L has been generally increasing approximately exponentially from its zero age main sequence value of about 70% of its value today. Accepting the view^{7,40} that an ice age is the equilibrium state of the climate when L is about 5% lower than its present value leads to the conclusion that the Earth was continuously glaciated until about 10^9 yr ago. It is essential to the hypothesis, therefore, that at least one of the terrestrial factors controlling the climate has a natural timescale comparable with or longer than the time taken for L to drop subsequent to the Sun being mixed. Theoretical estimates are difficult to make but there is some empirical evidence suggesting that during the Tertiary the climate underwent substantial variations on a timescale of several million years⁴¹⁻⁴⁴. It seems most likely that these variations were produced either by solar variations, which would at least confirm the conclusion that the climate is strongly influenced by the Sun, or by some factor of confluence

of factors intrinsic to the Earth, which would establish the existence of a sufficiently long natural timescale.

It is also necessary to ask whether the 5% drop in L is enough to produce an ice age. Theoretical discussions^{7,40,45} suggest it is, but because these are deficient at least so far as they do not predict the long term stability of the climate, they cannot be relied on. The supporting empirical evidence is that the relatively short period low amplitude fluctuations in the insolation at a particular latitude arising from precession and planetary perturbations of the Earth's motion have been correlated with the fluctuations in the extent of glaciation over the last 3×10^5 yr (refs. 46-49). Because the amplitudes of the fluctuations in the total annual insolation are typically only about 1%, one can at least say that it is not implausible that a 5% fluctuation is sufficient to bring about a major ice age.

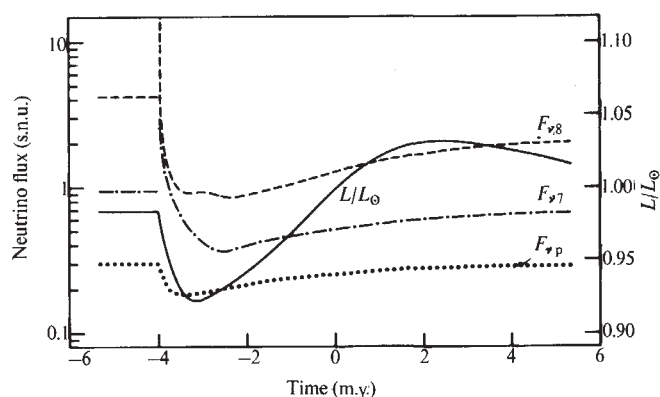


Fig. 1 Time variations of the surface luminosity of the Sun, in units of its present value, and of the neutrino fluxes $F_{\nu p}$, $F_{\nu 7}$ and $F_{\nu 8}$ measured in s.n.u. (The neutrino capture cross-sections were taken from Bahcall³⁶.) The first minimum in $F_{\nu 8}$ occurs soon after the convective core begins to recede, the second when the central temperature is also a minimum. Time is measured in units of m.y.

Accepting this picture leads one to expect the beginning of the Pleistocene epoch to be located near the luminosity minimum at about 3 m.y. BP, when glaciation in midlatitudes began⁵⁰ (Fig. 1). The beginning of the depression of L , at about 4 m.y. BP, is then concomitant with the estimated onset of the Antarctic glaciation⁵¹. Because the beginning of the Pleistocene epoch was less than a solar Kelvin-Helmholtz time ago, the Sun is not at present in thermal equilibrium. This accounts at least in part for the discrepancy between the theoretically predicted neutrino flux and the upper bound measured by Davis and his collaborators.

After the core is mixed the Sun's evolution temporarily reverses its direction in the H-R diagram, envelope convection becomes more vigorous and presumably solar activity and the strength of the solar wind increase. The flux of high energy particles incident on the Earth and hence the production of radioactive nuclei in the atmosphere is therefore modified. One might hope that calibrating ${}^{14}\text{C}$ dating against other methods, for example, would show this, but during the comparatively short times over which this is possible other perturbing influences, such as the changes in the geomagnetic field, dominate⁵². Alternatively one might expect to unravel a direct record of the cosmic ray flux by analysing nuclear tracks in rocks, but this has not yet been possible⁵³.

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Dislocations in Tobacco Mosaic Virus

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Crystal dislocations occur in polymerized protein from TMV. Their several possible functions in rearranging protein subunits and feeding in RNA into assembling TMV are described.

WHEN Watson introduced the word "dislocation" into the literature of tobacco mosaic virus (TMV) he had in mind the crystal defect well known to crystal physicists¹. Since then the term has all but lost its original meaning and is now used to describe steps and displacements of various kinds in the protein of TMV²⁻⁸. This would be unimportant were it not that true crystal dislocations occur not only in TMV but also—as I describe below—may perform important functions in its assembly. My purpose here is to examine these possible functions.

Polymerized protein from TMV occurs as hollow rods in which the protein molecules are arranged in paired rings ("stacked disks") or along a simple or double helix²⁻¹³.

Occasionally one finds a rod, part of which is one of these three forms while the remaining portion is one of the other two (refs. 14-16, and ref. 7, Fig. 1.4)¹⁴⁻¹⁶. Markham^{14,15} pointed out that such a rod of mixed character contained a dislocation. The dislocation lies in the boundary between the two portions, at the termination of a helical row of protein subunits (Fig. 1), and is edge in character. In the mechanism of assembly of TMV proposed by Klug and co-workers^{3,4,6,13}, disks attach to the end of the growing helical rod; before they convert to simple helices the assembly contains a grown-in edge dislocation as in Fig. 1, but closer to the end of the rod (Fig. 4a). Protruding from the assembly at or near the dislocation is the RNA still to be fed in^{3,4,6,13}. Butler and Klug show a structure³ (their Fig. 5) which can be understood only if dislocations of yet another kind (Fig. 4c) occur and are mobile.

What are the consequences of the presence of these dislocations? Clues are provided by the following properties of dislocations: first, as a dislocation moves there is a continuous structural rearrangement in its neighbourhood; second, dislocations are attracted by free boundaries, such as the end of the rod; and third, they represent pores through which transport is greatly facilitated^{17,18}.

Structural Rearrangement

The structural rearrangement near a moving dislocation is essentially a wave of limited and local slip between neighbouring rows of protein subunits: one row slides relative to a neighbouring row not uniformly along its length but in a caterpillar-like fashion. The line between the two sliding rows is the slip or glide line of the dislocation. The glide line of the dislocation in Fig. 1 is a long-pitched, right-handed helix as shown schematically by the dashed line in Fig. 2. As the dislocation glides along its glide line slip occurs locally between subunits on either side of the line. If, for example, movement is in the direction of the arrow in Fig. 2, then the helical portion of the rod grows at the expense of the portion consisting of stacked disks. In other words a moving edge dislocation is acting as a disk-to-helix converter. A moving dislocation brings about a topological change in the arrangement of the protein subunits¹⁹. Other non-topological changes may accompany the movement of the dislocation¹⁹: an example is the loss of the pairing interaction between rings, described by Klug and co-workers^{6,9},

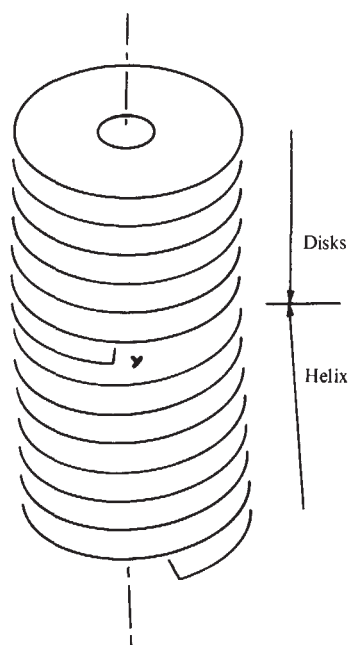


Fig. 1 Schematic representation of a rod of polymerized protein from TMV. Part consists of stacked disks while the rest is a simple helix. At the termination of the helix in the boundary between the two portions is a dislocation shown by the dislocation symbol. The presence of the dislocation causes the rod to be kinked.

that accompanies the conversion of disks to simple helix. A quantitative analysis of the passage of dislocations through cylindrical crystals, that is through structures such as the polymerized protein of TMV, is given elsewhere^{19,20}. Slip between rows of subunits has been suggested by several authors both in TMV²¹ and in other viruses^{19,22} while we¹⁹ have argued that the dislocation is the carrier of slip in such structures.

Further evidence that slip occurs in TMV is provided by the fact that defects in helical rods produced after the pH of a solution of protein is suddenly dropped are slowly annealed out with time^{2,5-8}. Annealing in ordinary crystals involves dislocations^{17,18} and it is likely that it also does so here. The defects in the rods appear to be vacancies, or clusters of vacancies, which can be removed by dislocations in a number of ways. A nick or vacancy in a helical rod is shown schematically in Fig. 3. The vacancy can be removed by slip along the helical dashed line. The rod becomes twisted or, rather, the part of the rod above the vacancy suffers a small, right-handed screw displacement about the axis of the rod relative to the rest of the rod. A vacancy can be treated as a

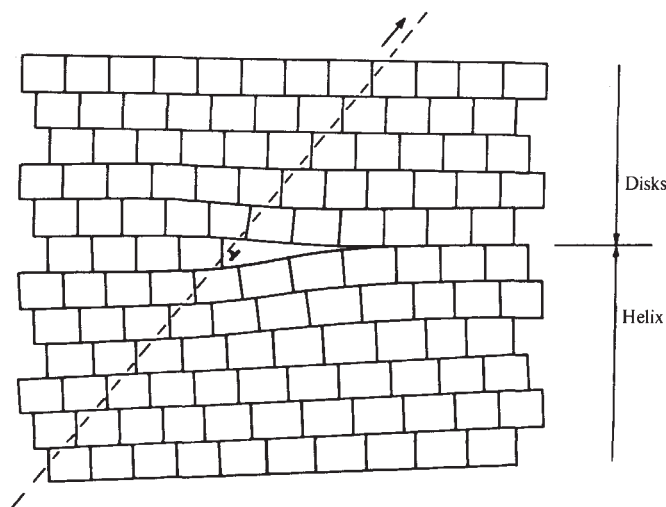


Fig. 2 Schematic representation in greater detail of the neighbourhood of the dislocation of Fig. 1. Again the position of the dislocation is shown by the dislocation symbol. The dashed line, a long-pitched right-handed helix on the rod, is the glide line of the dislocation. It is the line along which the dislocation can glide resulting in caterpillar-like slip between subunits on either side. As the dislocation glides in the direction of the arrow, local slip converts rings or disks to turns of the simple helix.

pair of edge dislocations as in Fig. 3. Slip along the dashed line of Fig. 3 occurs by the movement towards each other, and eventual annihilation, of the two dislocations along the dashed line, their mutual glide line. As described below, the existence and mobility of this type of dislocation are implied by Fig. 5 of ref. 3. Vacancies can also be removed by a process known as dislocation climb, in which an edge dislocation moves in a direction perpendicular to its glide line^{17,18}. It is likely that the defects in the rods are removed by a process consisting of both climb and glide of dislocations. A general treatment of twisting of rods due to movement of dislocations is given elsewhere²³.

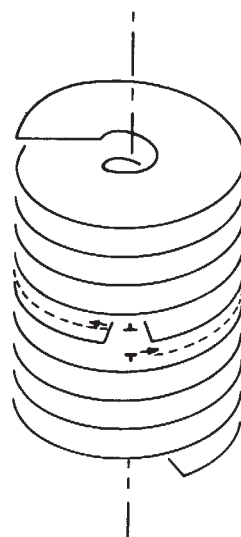


Fig. 3 Schematic representation of a vacancy or nick in a helical rod. The vacancy can be removed by slip along the dashed helical line. Such slip results in one portion of the rod suffering a screw displacement about the axis of the rod relative to the rest of the rod. The vacancy is represented by a pair of opposite dislocations with the same glide line, the dashed helical line. Slip is brought about by glide of the dislocations in the directions indicated along the glide line. The dislocations finally meet and annihilate resulting in a defect-free rod.

Rods of polymerized protein arranged in stacked disks appear to be very stable; they do not convert to the simple-helical form^{2,8,12}. This can be explained by the fact that the rods, being free of defects or nearly so, have no active sources to supply the dislocation necessary to bring about the conversion.

Dislocations during Virus Assembly

The completed TMV virion arises from the interaction of double-layered disks, with seventeen subunits per layer, with one strand of RNA^{3,4,6}. Assembly apparently commences with the interaction of a disk with a specific sequence of about fifty nucleotides at the 5' end of the RNA^{3,4,6}. More disks are added successively to the growing assembly with each converting to short portions of a simple helix. The RNA feeds itself in to become sandwiched between neighbouring turns of the helix. There is no evidence that short helical portions of protein, that is those structures known as "lock-washers"^{2,3,5-7,11}, occur freely under any conditions in cells of the tobacco plant^{2,5,6}. This suggests that it is only as a result of the interaction of the disk with the rest of the assembly that the disk is transformed. In terms of dislocations this is readily understood: the grown-in dislocation (Fig. 4a) already mentioned is attracted to the end of the rod and passes through the disk converting it to a simple helix with $16\frac{1}{2}$ subunits per turn (Fig. 4b) as in the rest of the assembly. The RNA remains protruding from the protein through a "ruck". The passage of the dislocation through the disk results in the displacement represented by arrow *A* in Fig. 4a. This mechanism is essentially the same as that described by Butler and Klug^{4,6}.

Insertion of RNA

Elsewhere³ the same authors describe a mechanism that at first sight may appear similar but is in fact different. The disk converts to a helix (Fig. 4c) by means of a shorter displacement represented by arrow *B* in Fig. 4a. What Butler and Klug³ do not make clear is that the helix produced by such a displacement necessarily has about $17\frac{1}{2}$ subunits per turn while the rest of the assembly still has $16\frac{1}{2}$ per turn. The difference must be

accommodated by a dislocation whose glide line is the simple helix between the turns of the helix formed by the protein subunits. The dislocation is shown at the point of protrusion of the RNA in Fig. 4c. It is of the same type as the upper dislocation in Fig. 3. The glide line is defined by the strand of RNA in the assembling structure. In this process the grown-in dislocation of Fig. 4a dissociates into two dislocations: one, on a long-pitched, left-handed, helical glide line, leaves the structure in a direction antiparallel to the arrow *B* converting the disk to a helix with $17\frac{1}{2}$ subunits per turn, and the other, on a short-pitched, right-handed, helical glide line, has the RNA protruding from it as in Fig. 4c. The extra subunit in the turns above the dislocation causes those turns locally to be under a circumferential compressive stress which may be partly relieved if those turns bulge axially as represented in Fig. 5. A pore is formed at the dislocation. Dislocations are paths through which transport is greatly facilitated^{17,18,24}. Thus it seems possible that the dislocation glides along its glide line in the direction of the arrow *A* in Fig. 5 converting the last two turns to $16\frac{1}{2}$ subunits per turn and continuously feeding in the RNA. The driving force has two reinforcing components: the chemical force which arises from the stronger attractive interaction that presumably exists between the turns in the presence of RNA at *B* in Fig. 5 than in the absence of RNA at *C*, and the mechanical force attracting the dislocation to the free end.

It is quite possible for the RNA to protrude at some point other than that shown in Fig. 4a. It may already have worked its way into place for part or all of another turn before the disk arrives. Wherever the point of protrusion, however, there is in all likelihood an attractive interaction between the point and a dislocation of the type shown in Fig. 5. Thus the dislocation may be formed as in Fig. 4c. It will then be attracted to the point of protrusion of the RNA just as in ordinary crystals dislocations are attracted to impurities^{17,18}. The dislocation and the point coalesce and the process continues as before. If the RNA is already in place for most or all of another turn before the disk arrives, then it is likely that the grown-in dislocation of Fig. 4a passes through the first ring of the disk in a direction parallel to arrow *A* and then dissociates

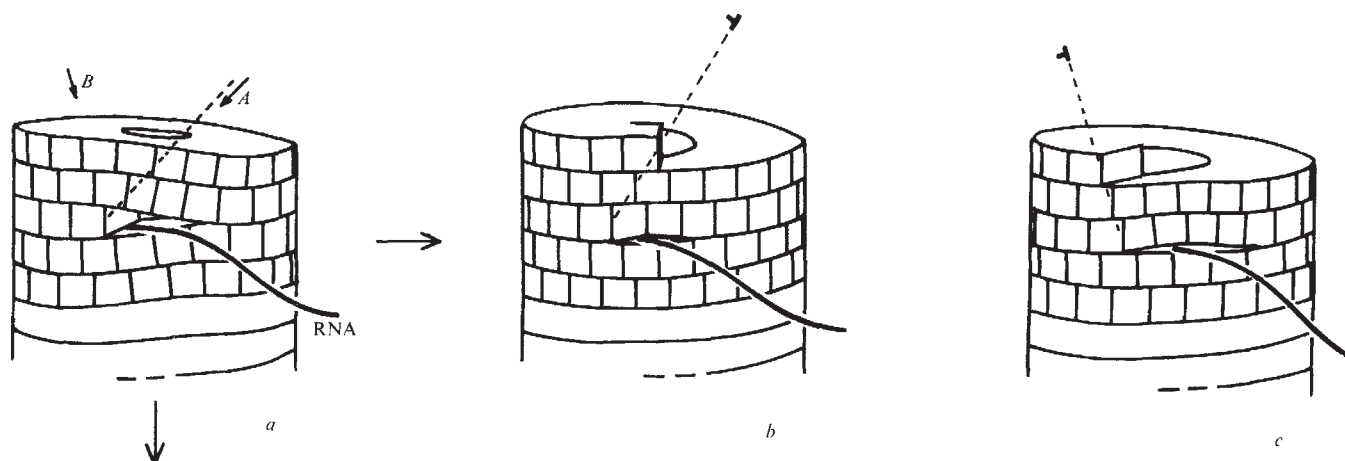


Fig. 4 The interaction of a disk with an assembling particle of TMV. In *a* the disk has added to the assembly and become distorted. A grown-in dislocation results at the termination of the helix adjacent to the disk. The dislocation is essentially identical to the dislocations in Figs. 1 and 2. As in Fig. 2 its glide line is the long-pitched right-handed helix shown by the dashed line. The RNA is shown protruding from the rod at the dislocation. If the dislocation leaves the assembly along its glide line, then it converts the disk to simple helix with about $16\frac{1}{2}$ subunits per turn as in *b*. The result is slip represented by the arrow *A*. The RNA remains protruding at a ruck. In *c* the grown-in dislocation of *a* dissociates into two dislocations, one which glides out on a left-handed helical glide line and another which is situated at the point of protrusion of the RNA in *c*. (Its presence is indicated by the fact that there is one more subunit per turn above the dislocation than below it.) The first dislocation results in a displacement represented by *B* in *a* and the conversion of the disk with 17 subunits per ring to nearly two turns of a helix with $17\frac{1}{2}$ subunits per turn. The second dislocation glides out of the rod along a short-pitched helix while continuously feeding in the RNA and reducing the number of subunits per turn to $16\frac{1}{2}$ as in the completed virion. It is quite possible that transition to *b* or *c* occurs while the disk is attaching itself to the assembly: in that case *a* does not develop fully. The process resulting in *b* is similar to the mechanism proposed by Butler and Klug^{4,6} while that resulting in *c* is similar to their mechanism described in ref. 3.

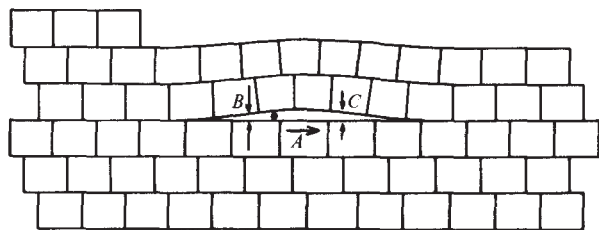


Fig. 5 Detail of the dislocation of Fig. 4c. The point of protrusion of the RNA is shown by the heavy dot. The compressive stress due to the presence of the dislocation is partly relieved by buckling of two turns in the axial direction. As the dislocation glides, caterpillar-like, in the direction of the arrow A, it feeds in the RNA continuously. Part of the driving force for movement of the dislocation arises from the greater attraction between turns at B in the presence of RNA than at C in the absence of RNA. The glide line of the dislocation is the same as that for the dislocations of Fig. 3.

into two dislocations as before, one leaving in a direction anti-parallel to the arrow B while the other traps the point of protrusion of the RNA and glides to the free end feeding in the nucleic acid as in Fig. 5 but between the next pair of turns. The position of the point of protrusion of the RNA after the disk has arrived presumably depends on the degree of supersaturation of the free disks in the cytoplasm near the assembling structure. For a sufficiently high supersaturation the RNA will protrude as in Fig. 4a. For lower supersaturations the RNA may protrude at any point up to another full turn further on.

It appears therefore that the RNA is fed into the growing assembly either through a ruck, as in Fig. 4b and as suggested by Klug and co-workers^{3,4,6}, or through a dislocation as in Fig. 4c. The dislocation is favoured, however, by a number of factors. First, it is known to be a path through which transport is greatly facilitated, which implies that the dislocation provides less resistance for the movement of the RNA into the assembly than does the ruck. Second, the dislocation is attracted to the free end of the assembly thus contributing to the driving force for the incorporation of the RNA. The ruck, representing a type of interstitialcy, would be attracted to the end but by a very much smaller force. Third, dislocations do actually occur in polymerized protein of TMV and evidence has been presented in support of their ability to move. I know of no evidence that rucks occur.

When Watson introduced the term dislocation into the literature of TMV he did not suggest that dislocations actually occurred or were involved in TMV in any way at all but merely pointed out the analogy between a possible mechanism of assembly of TMV and growth at a step leading from the point of emergence of a screw dislocation in a conventional crystal¹. Since then the step at the end of the simple helical form of protein has sometimes been mistakenly called a dislocation and even a screw dislocation^{7,21}. Often, too, the helical form itself is said to be dislocated or contain a dislocation²⁻⁸. It is true that the helical forms contain what we refer to as a "global defect"^{25,26}, one component of which is a dislocation, but then so does the stacked-disk form and any other form for that matter. (With this type of dislocation there is no terminating row.) In this sense all forms are dislocated and there is no point in singling out the helical forms. By saying that a structure is dislocated, or defective in any other way, one implies some perfect or defect-free reference structure. The most convenient reference for cylindrical crystals is the planar array obtained by flattening a portion of the cylindrical protein shell and extending it repetitively in the plane without limit by adding more subunits¹⁹. This reference is closely related to the radial projection²⁷. Implicit in the use of the term dislocation by Klug and co-workers²⁻⁸ appears to be that the disk or stack of disks is the reference. In normal usage a dislocation is defined in terms of a translational displacement while the displacement converting the disk to a helix, represented for example by arrows A or B in Fig. 4a, is actually a screw

displacement. Defects resulting from such displacements are known as dispirations^{25,28}. A helical form then is a dispirated disk or stack of disks²⁵. A "partial wedge disclination"^{18,25} in a ring of protein subunits from TMV has been referred to as an edge dislocation²⁹; it is the source of the strain energy, in a disk, described by Durham and Klug¹². The imprecise usage of the term dislocation in these and other contexts is discussed more fully elsewhere^{19,25}.

The different types of displacements that have been described by Klug and co-workers in connexion with TMV can, I believe, be analysed in terms of dislocations. In recognizing the relationship between these displacements and dislocations, one is provided with an additional and powerful tool, the well-developed theory of dislocations, which can then be brought to bear on the problem of assembly of TMV. Confusion is bound to arise if the term dislocation continues to be used loosely as at present. I suggest, therefore, that it be used only in the original sense as used by Watson¹, that is in the sense used by crystal physicists.

Dislocations and related defects are widely distributed in biology (see refs. 25, 30-32) and I believe that they will prove to be important in many processes involving movement, morphogenesis and mechanochemical conversion of energy. With reference to TMV they are found in polymerized viral protein, they are probably involved in the annealing out of defects produced in rods when the pH of solutions of protein is lowered, they can convert one form of polymerized protein to another, they convert disks to turns of a simple helix after or while the former attach to the end of the latter in assembly of TMV, and they may be the moving point in the protein through which RNA is continuously fed into the assembling virion.

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LETTERS TO NATURE

PHYSICAL SCIENCES

The Universe as a Black Hole

SINCE Einstein applied his general theory of relativity to study the structure of the universe as a whole¹, cosmologists have wondered if the universe is geometrically closed or open. Neither theory nor observation has been able to settle this question unambiguously. Several authors have hoped that the universe may after all be a closed, yet unbounded, system. This would solve many problems regarding the nature and origin of the universe, and would fit many of the observations of distant sources made at radio, optical and other wavelengths². Here I demonstrate that the universe may not only be a closed structure (as perceived by its inhabitants at the present epoch) but may also be a black hole, confined to a localized region of space which cannot expand without limit.

An object of mass M is a black hole if it is confined to a region of space bounded by a surface of area $4\pi R_s^2$, where R_s is the Schwarzschild radius of the object. By definition, the Schwarzschild radius is characterized by the vanishing of the element g_{44} of the metric tensor g_{ik} , and is (ordinarily) given by $2GM/c^2$. For a typical star, R_s is of the order of 10^5 – 10^6 cm; for the universe as a whole, R_s would be of the order of 10^{28} cm. Because the linear dimensions associated with the universe are also of the order of 10^{28} cm, the question arises: Is the universe itself a black hole? To investigate this question, the customary view of the universe, which is necessarily internal, is not sufficient; it has to be supplemented with an external view—I assume that there exists, outside our universe, an external world from which one may take a “detached” look at our universe. It turns out that these two views are not only mutually compatible but also show considerable similarities. Both views are presented here.

For studying the space-time structure of the universe from inside, we regard it as a smeared-out system with uniform mass density $\rho(t)$ and radius of curvature $R(t)$. The geometry of the space-time, in a comoving frame of reference, is governed by the Robertson-Walker metric^{3,4}

$$ds^2 = \{R(t)\}^2 \frac{dx^\alpha dx^\alpha}{\left(1 + \frac{\kappa}{4}(x^\alpha x^\alpha)\right)^2} - c^2 dt^2 \quad (\alpha = 1, 2, 3) \quad (1)$$

where κ ($= +1, 0$ or -1) is the index of curvature. $R(t)$ is a function of the pressure p and density ρ of the universe and is determined by Einstein's field equations. For $p \ll \rho c^2$

$$\left(\frac{dR}{dt}\right)^2 = c^2 \left(\frac{1}{3} \Lambda R^2 + \frac{C}{R} - \kappa\right) \quad (2)$$

where Λ is the cosmological constant and C is the mass constant of the universe

$$C = 8\pi G(\rho R^3)/3c^2 \quad (3)$$

Equation (2) leads to a variety of cosmological models^{4,5}. I wish to concentrate on the case $\kappa = +1$ which corresponds to a space of positive curvature—the three-dimensional analogue of the surface of a sphere. In this case

$$\left(\frac{dR}{dt}\right)^2 = c^2 \left(\frac{1}{3} \Lambda R^2 + \frac{C}{R} - 1\right) \geq 0 \quad (4)$$

At the present epoch the universe is in a phase of expansion and the rate of expansion seems to be on the decrease;

accordingly

$$H_0 = \left(\frac{\dot{R}}{R}\right)_{t=t_0} = \frac{c}{R_0} \left(\frac{1}{3} \Lambda R_0^2 + \frac{C}{R_0} - 1\right)^{1/2} > 0 \quad (5)$$

and

$$q_0 = \left(-\frac{\ddot{R}R}{\dot{R}^2}\right)_{t=t_0} = -\frac{\left(\frac{1}{3} \Lambda R_0^2 - \frac{C}{2R_0}\right)}{\left(\frac{1}{3} \Lambda R_0^2 + \frac{C}{R_0} - 1\right)} > 0 \quad (6)$$

Here, H_0 and q_0 denote the present-day values of the Hubble parameter H and the deceleration parameter q . Equations (4)–(6) imply that at a later epoch the function $R(t)$ may require a maximal value $R_{\max}$, which will be determined by the conditions

$$\left(\frac{1}{3} \Lambda R_{\max}^2 + \frac{C}{R_{\max}} - 1\right) = 0 \quad (7)$$

and

$$\left(\frac{2}{3} \Lambda R_{\max} - \frac{C}{R_{\max}^2}\right) < 0 \quad (8)$$

Several possibilities arise here:

$\Lambda \leq 0$. In this case we have only one real solution

$$R_{\max} = \frac{2}{|\Lambda|^{1/2}} \sinh \left\{ \frac{1}{3} \sinh^{-1} \left(\frac{|\Lambda|}{\Lambda_c} \right)^{1/2} \right\} \quad \left(\Lambda_c = \frac{4}{9C^2} \right) \quad (9)$$

which decreases monotonically from the value $R_{\max} = C$ to $R_{\max} = 0$ as Λ decreases from 0 to $-\infty$.

$0 \leq \Lambda \leq \Lambda_c$. In this case all three solutions are real

$$R_{\max} = \frac{2}{\Lambda^{1/2}} \cos \left\{ \left(\psi + \frac{\pi}{6} \right), \left(\psi + \frac{5\pi}{6} \right), \left(\psi + \frac{9\pi}{6} \right) \right\} \quad (10a, b, c)$$

where

$$\psi = \frac{1}{3} \sin^{-1} (\Lambda/\Lambda_c)^{1/2} \quad (0 \leq \psi \leq \pi/6) \quad (11)$$

Solution (10b) is negative, and may be rejected at once. Solution (10a) corresponds to a minimum rather than a maximum; so it is of no interest at present. The only relevant solution in this case is (10c), which may also be written as

$$R_{\max} = \frac{2}{\Lambda^{1/2}} \sin \left\{ \frac{1}{3} \sin^{-1} \left(\frac{\Lambda}{\Lambda_c} \right)^{1/2} \right\} \quad (12)$$

It increases monotonically from the value $R_{\max} = C$ to $R_{\max} = \frac{3}{2} C$ as Λ increases from 0 to Λ_c .

$\Lambda > \Lambda_c$. In this case again there is only one real solution, but it is always negative. Accordingly, $R(t)$ never acquires a maximal value.

I now consider an external view of the universe. Because the expansion of the universe is isotropic, space-time in its exterior will be governed by a static, Schwarzschild metric (modified by the presence of the cosmological term^{5,7})

$$ds^2 = \frac{dr^2}{1 - \frac{2\mu}{r} - \frac{1}{3} \Lambda r^2} + r^2(d\theta^2 + \sin^2 \theta d\phi^2) - \left(1 - \frac{2\mu}{r} - \frac{1}{3} \Lambda r^2\right) c^2 dt^2 \quad (13)$$

where

$$\mu = \frac{4\pi G}{c^2} \int_0^R \rho(r) r^2 dr = \frac{4\pi G}{3c^2} (\rho R^3) \quad (14)$$

The element g_{44} of the metric (13) is given by the expression

$$\left(-1 + \frac{C}{r} + \frac{1}{3} \Lambda r^2 \right)$$

where C is the same constant as in equation (3). The Schwarzschild radius of the universe is therefore determined by the conditions

$$g_{44} \left(r \begin{matrix} \geq \\ < \end{matrix} R_s \right) \begin{matrix} \leq \\ > \end{matrix} 0 \quad (15)$$

which imply that

$$\left(-1 + \frac{C}{R_s} + \frac{1}{3} \Lambda R_s^2 \right) = 0 \quad (16)$$

and

$$\left(\frac{\partial g_{44}}{\partial r} \right)_{r=R_s} = \left(-\frac{C}{R_s^2} + \frac{2}{3} \Lambda R_s \right) < 0 \quad (17)$$

Conditions (16) and (17) for R_s are identical with conditions (7) and (8) for $R_{\max}$. This is true only for $\kappa = +1$, which therefore seems the natural choice. Solutions for R_s are therefore identically the same as for $R_{\max}$ —irrespective of the values of the parameters C and Λ , except that for $\Lambda > \Lambda_c$ neither R_s nor $R_{\max}$ exists. Now, the Schwarzschild radius of the universe is obtained from the static metric in the exterior of the universe while the maximal value of the function $R(t)$ was obtained from the nonstatic metric in the interior of the universe. The fact that the two, whenever they exist, are identically equal can hardly be a coincidence. It is therefore tempting to suggest that the identity $R_s = R_{\max}$ is fundamental to the structure of the universe; accordingly, we must have

$$\kappa = +1 \text{ and } \Lambda \leq \Lambda_c \quad (18)$$

It follows that at any epoch

$$R(t) \leq R_s \quad (19)$$

which means that the universe is indeed in a black hole.

At present, the largest sphere that can be drawn in the universe has a surface area $4\pi R_s^2$. As expansion goes on, this may approach the maximal value $4\pi R_{\max}^2$ ($\equiv 4\pi R_s^2$). Being inside a black hole, we cannot hope to "shoot through" the Schwarzschild surface; we may approach it in infinite time ($\Lambda = \Lambda_c$) or in a finite time ($\Lambda < \Lambda_c$). In the latter case, the universe must retrace its steps and proceed along a phase of contraction, eventually producing densities where present understanding of physics breaks down (with, perhaps, a further expansion from the "primaeval" matter and so on, in an endless cycle of pulsations).

Some of the immediate consequences of this picture may be expressed in terms of inequalities which must be satisfied by the various parameters characterizing the universe. Arising from conditions (18), these inequalities provide lower and upper bounds for the parameters Λ and ρ_0 , and a lower bound for the parameter R_0 , in terms of the observable quantities H_0 and q_0 ; so these parameters can be estimated from the kinematics of the universe alone. The numerical values resulting from these inequalities may not be very accurate because of the errors in the observed values of H_0 and q_0 (which, at the present time, may be as large as 20–40% (ref. 6)). But using $H_0 = 75 \text{ km s}^{-1} (\text{Mpc}^{-1})$ and $q_0 = 1$ as "representative values"

$$-6.7 \times 10^{-57} \text{ cm}^{-2} < \Lambda \leq \Lambda_c \leq 1.0 \times 10^{-57} \text{ cm}^{-2} \quad (20)$$

$$1.5 \times 10^{-29} \text{ g cm}^{-3} < \rho_0 \leq 2.3 \times 10^{-29} \text{ g cm}^{-3} \quad (21)$$

$$R_0 \geq 1.1 \times 10^{28} \text{ cm} \quad (22)$$

The ranges in which Λ and ρ_0 may lie are narrow.

Apart from these immediate consequences, there are deeper

implications as well. For instance, we are now faced with several questions: How did the universe come to be a black hole—through a gravitational collapse, followed by a phase of expansion? In the cosmos, which includes the exterior as well as the interior of the universe, can our universe be unique? If not, what would its status be *vis-a-vis* other such structures in the cosmos? Investigation of these and other related questions, including the possible existence of an hierarchy of black holes, is clearly a matter of some importance.

As for our own universe, the concept of the Schwarzschild radius itself seems to be of considerable significance. Its relevance to other realms of physical phenomena will be discussed in a subsequent communication.

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Orbital Eccentricity of Mercury and the Origin of the Moon

A NUMBER of mechanisms for the formation of the Moon have been suggested; fission of the Earth, precipitation in a hot gaseous silicate atmosphere, independent formation in orbit about the Earth, and independent formation elsewhere in the solar system followed by capture by the Earth¹. Although the last of these mechanisms has been admitted to be improbable by its proponents, they have shown that it is by no means impossible dynamically². The principal objection to this mechanism is the strange composition of the Moon. It has been recognized for many years that the low mean density of the Moon implies that it is highly deficient in metallic iron. The lunar exploration programme has also shown that the Moon is much more deficient than the Earth in the more volatile of the condensable elements. Because of the apparent difficulty of satisfying these composition constraints in a theory in which the Moon is formed elsewhere in the solar system, I have tended to favour the other mechanisms mentioned above^{3,4}.

The situation now appears to be changed as a result of recent work by D. L. Anderson^{5,6} (presented in most fully developed form at the IAGC). He justifies the postulate that, in addition to the above chemical abnormalities, the deep interior of the Moon is probably also very deficient in magnesium silicates, and that it is remarkably free of iron oxide in the silicate materials. Such a Moon would be close to the melting point in much of the deeper interior, yet its electrical conductivity would not exceed the bounds placed by analysis of lunar magnetometer measurements⁷. Anderson postulates that the Moon has the same basic composition as the calcium and aluminium-rich silicates in the inclusions in the Allende carbonaceous chondrite, which are believed to be very high temperature condensates within a cooling primitive solar nebula⁸.

As a hot gas of solar composition cools, at a pressure of

the order of 10^{-3} atmosphere, the first major constituents to condense consist of calcium, aluminium, and titanium oxides and silicates, such as corundum, perovskite, and melilite. As the gas continues to cool, metallic iron, alloyed with nickel, will be precipitated, and shortly thereafter magnesium silicates will condense⁹.

I have recently constructed models of the primitive solar nebula, in which the internal temperatures and pressures are related by the adiabat likely to be produced on compression of the gas as it reaches the adiabatic state in the collapse of an interstellar gas cloud¹⁰. In such models, the temperature increases progressively toward the centre of the primitive solar nebula, and the interstellar grains are completely evaporated only near the central spin axis. I have also concluded that planetary bodies should accumulate very rapidly within such a primitive solar nebula¹¹. In this picture it is a natural conclusion that the high mean density of the planet Mercury results from its accumulation from material in the primitive solar nebula in the temperature range in which metallic iron has condensed but the magnesium silicates have not. Lewis has independently reached this same conclusion¹².

Grossman⁹ has pointed out that the calcium and aluminium-rich inclusions in the Allende meteorite can be formed inside the region of iron condensation in the primitive solar nebula, because their compositions are consistent with the very highest temperature condensates. If Anderson is correct in his conclusion that the bulk composition of the Moon resembles that of the Allende inclusions, then the natural place for the formation of the Moon in the solar system is inside the orbit of Mercury, through planetary accumulation from the condensed material to be found there.

This explains the anomalous large eccentricity (0.206) of the orbit of Mercury. The temperature gradient in the primitive solar nebula was probably steep enough for the orbital radii at which the Moon and Mercury were formed to differ by a much smaller relative amount than do the orbits of other neighbouring planets within the solar system. Thus gravitational perturbations of the orbits of the two bodies would probably accumulate until a close approach took place, at which a very large modification in the elements of the Moon's orbit would become possible. If the modified orbit of the Moon were sufficiently great to allow it to approach the Earth, then gravitational capture of the Moon by the Earth would become possible, even if improbable.

The semi-major axis of the orbit of Mercury is 0.387 astronomical units (a.u.). The eccentricity of this orbit is 0.206. Thus the aphelion distance of the orbit is at 0.467 a.u. It is reasonable to suppose that Mercury may have originally been in a circular orbit having a radius of 0.467 a.u.

The total energy, kinetic and potential, of a planet in orbit about the Sun is $GMm/2a$, where M is the mass of the Sun, m is the mass of the planet, and a is the semi-major axis of the orbit. In transforming from a circular orbit with $a=0.467$ a.u. to the present orbit, the total energy of the orbit of Mercury would be decreased (algebraically) by 6.38×10^{38} erg. It is interesting to see what consequences would follow if this amount of energy should be transferred to the orbit of the Moon.

If the Moon started in a circular orbit at about 0.33 a.u. from the Sun, and was perturbed into an orbit having this distance as perihelion and an aphelion distance of about 1 a.u., then the semi-major axis of the transformed orbit will be about 0.67 a.u. The change of orbital energy required to produce this orbit is less than the above change in the orbital energy of Mercury. In fact, an initial circular orbit for the Moon could be situated as close to the Sun as 0.28 a.u. and be transformed into an orbit with a perihelion at this distance and an aphelion at the distance of the Earth.

Thus the chemical evidence pointing toward the formation of the Moon inside the orbit of Mercury is compatible with the dynamical requirement that the transformation of an initial circular orbit of Mercury into the present orbit of

relatively large eccentricity should provide enough energy to transform the orbit of the Moon in such a way that the Moon crosses the orbit of the Earth.

The precise figures used in this analysis should be regarded as only illustrative. If the orbital perturbations occurred before the primitive solar nebula had been dissipated, the additional mass would modify the gravitational potential so that it was not quite inversely proportional to the distance from the central spin axis, and these figures would require modification. Also, even after dissipation of the primitive solar nebula, the Sun was probably much more massive than at present, because the T Tauri phase of mass loss would only be beginning. This would modify the numbers but not their relative orders of magnitude.

The above considerations pose a reasonably well defined problem for workers on the dynamical problem of capture of the Moon by the Earth, but they do not give its solution. The suggested orbit of the Moon, as perturbed by Mercury, is likely to be transformed further through perturbations by Venus and the Earth, but even so the approaches to the Earth will occur at a significant hyperbolic velocity. The most recent calculations on the dynamical capture process² have not yet determined the range of hyperbolic velocities which can be tolerated in this process.

The Earth probably acquired much of its present content of the more volatile elements by sweeping up smaller bodies in the vicinity of its orbit after the dissipation of the primitive solar nebula. Because the abundances of these elements in the Moon are so low, the capture of the Moon by the Earth probably did not occur until after this sweeping action of the Earth was almost complete.

As the lunar exploration programme has progressed, there has been an increasing tendency to regard the Moon as a planetary body in its own right. The conclusions above seem to justify this judgment.

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Stratospheric Nitrogen Dioxide from Infrared Absorption Spectra

STRATOSPHERIC absorption due to NO_2 has been identified¹⁻³ in infrared spectra of the solar radiation recorded from balloon borne gondolas floating at mid-latitudes in the altitude range

of 30 km when the solar zenith angle is larger than 91° . In this particular geometric situation most of the absorption by the atmosphere takes place at altitudes smaller than float altitude over very long optical paths. This offers two great advantages: the sensitivity of the absorption method is maximum and the quantitative interpretation is easier because absorption takes place mostly at constant total pressure, constituent concentration and temperature where the solar radiation passes closest to the Earth's surface.

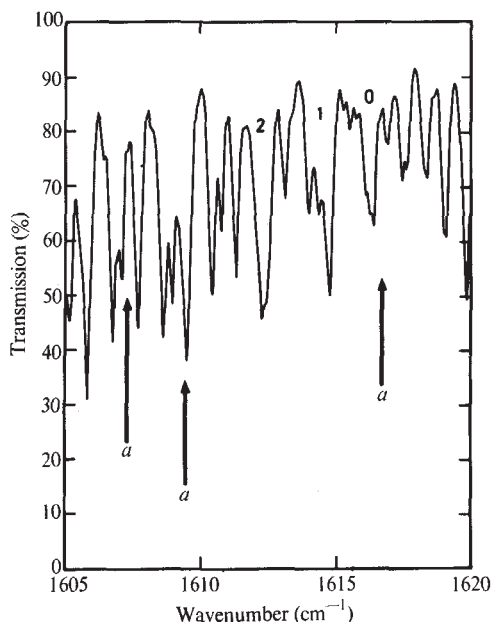


Fig. 1 Synthetic absorption spectrum of a portion of the v_3 band of NO_2 computed on the basis of the available spectroscopic data⁴ and calibrated in absolute value by means of the laboratory measurements of Guttman⁶; optical thickness: 0.014 atm cm; total pressure: 1.18 atm; temperature: 298 K. The position of water vapour lines is indicated (a), one of them overlapping the NO_2 absorption marked 0. Absorptions 1 and 2 are unaffected by water vapour and appear clearly in the atmospheric spectra.

A quantitative interpretation of the available data leading to stratospheric mixing ratios of NO_2 from the edge of the tropopause at 12 km up to 28 km is reported here. Synthetic spectra have been computed on the basis of the spectroscopic constants given for NO_2 by Olman and Hause⁴ using a Lorentz line full halfwidth of 0.16 cm^{-1} in STP conditions. The choice of this value is based on the work of Yamamoto and Aoki⁵. The line intensity was determined by comparing the integrated synthetic spectra with the integrated absorption measured in the laboratory by Guttman⁶. Fig. 1 shows the transmission versus wavenumber of a portion of the v_3 band computed for the laboratory conditions used by Guttman⁶. The positions of water vapour lines are shown at the wave numbers given by Benedict and Calfee⁷. One of these overlaps the NO_2 absorption feature numbered 0 on the figure. Absorptions numbered 1 and 2 are very well isolated; we have used chiefly this part of the spectrum, particularly well marked in the atmospheric recordings³, chiefly for the quantitative interpretation. The $v_1 + v_3$ band^{1,2} underlies very clearly the P branch of the v_3 CH_4 band. The mixing ratios by volume shown in Table 1 and Fig. 2 refer to the altitudes of the solar grazing rays and have been deduced from the NO_2 concentrations determined from the spectra and the total concentration given in the Mid-Latitude, Spring-Fall model of US Standard Atmosphere Supplements, 1966. The quoted errors correspond to lower and upper values of the amount of absorber in the optical path leading to computer generated spectra that could be distinguished from the observed atmospheric spectra.

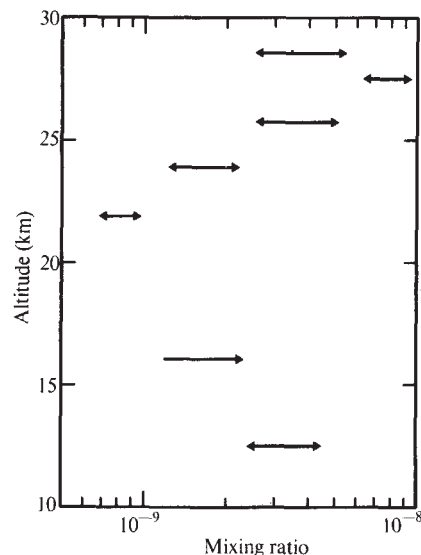


Fig. 2 Stratospheric NO_2 mixing ratio by volume.

We believe that the relatively high value observed at the lowest altitude is due to a tropospheric influence. The mixing ratio seems to increase from 20 to 28 km showing the presence of a stratospheric layer of NO_2 . This might be why no evidence for NO_2 has been found up to now in submillimetre stratospheric emission spectra⁸ taken from an altitude of 12 km at a zenith angle of 75° .

Table 1 Mixing Ratios relative to Zenith Angle

Balloon altitude (km)	Solar zenith angle	Grazing solar rays altitude (km)	NO_2 mixing ratio by volume	Observed band	Reference to spectra
29	94.1	12.5	$(3.4 \pm 1) \times 10^{-9}$	$v_1 + v_3$	(1, 2)
34	94.3	16.1	$< 2.3 \times 10^{-9}$	$v_1 + v_3$	(1, 2)
29.7	92.8	21.9	$(8.3 \pm 1.5) \times 10^{-10}$	v_3	(3)
29.7	92.4	23.9	$(1.7 \pm 0.5) \times 10^{-9}$	v_3	(3)
29.7	92.0	25.7	$(3.8 \pm 1.3) \times 10^{-9}$	v_3	(3)
29.7	91.5	27.5	$(7.8 \pm 1.5) \times 10^{-9}$	v_3	(3)
29.7	91.2	28.3	$(4.0 \pm 1.5) \times 10^{-9}$	v_3	(3)

Following the theoretical investigation made by Nicolet⁹ on the role of NO_2 on the stratospheric ozone, the values listed in Table 1 indicate an influence of natural nitrogen dioxide on O_3 . In sunset conditions as is the case for these determinations, NO_2 mixing ratios can be taken, within experimental errors, equal to NO_x .

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BIOLOGICAL SCIENCES

Further Investigations of the Transfer of Bomb ^{14}C to Man

MARKED temporal variations in the concentration of ^{14}C in the Earth's atmosphere have occurred during the past twenty years of testing nuclear weapons¹⁻⁴. The concentrations of ^{14}C assimilated by plants have been shown to provide an accurate record of these variations⁵⁻⁹, but concentrations of "bomb ^{14}C " reported at the same time for human tissues have differed slightly from these¹⁰⁻¹⁵.

Two factors have been considered as the major contributors to the general damping effect noted for human ^{14}C concentrations compared with atmospheric ^{14}C variations: first, the selective composition of the human diet and, second, the finite residence time for carbon in human tissues.

Our study has evaluated the mean dietary intake of ^{14}C by the population of the United Kingdom, and assesses the possible application of temporal changes in "bomb ^{14}C " concentrations as a tracer for human metabolic processes.

Concentrations of ^{14}C in selected dietary components were monitored from 1967 to 1969 (Table 1). Good agreement was found to exist between the ^{14}C concentrations in foods of terrestrial origin and the current atmospheric concentration. It would appear that the time involved for the transfer of carbon through the major pathways of the food chain is short, certainly within the interval between successive periods of maximum photosynthetic activity. The most significant dietary modification resulted from the low concentrations of ^{14}C in foods of marine origin. These latter values reflect the concentrations of ^{14}C reported for carbon dissolved in the oceanic euphotic zone^{16,17}.

On the assumption that 5% of the UK protein diet is derived from sea foods¹⁸ a first approximation of the rate of transfer of "bomb ^{14}C " to the population over the past decade was computed using the expression

$$D_t = 0.9 A_t + 0.05 O_t$$

where D , A and O were the respective concentrations of ^{14}C in the diet, atmosphere and oceans at time t . To allow for seasonal variations in atmospheric ^{14}C concentration, values

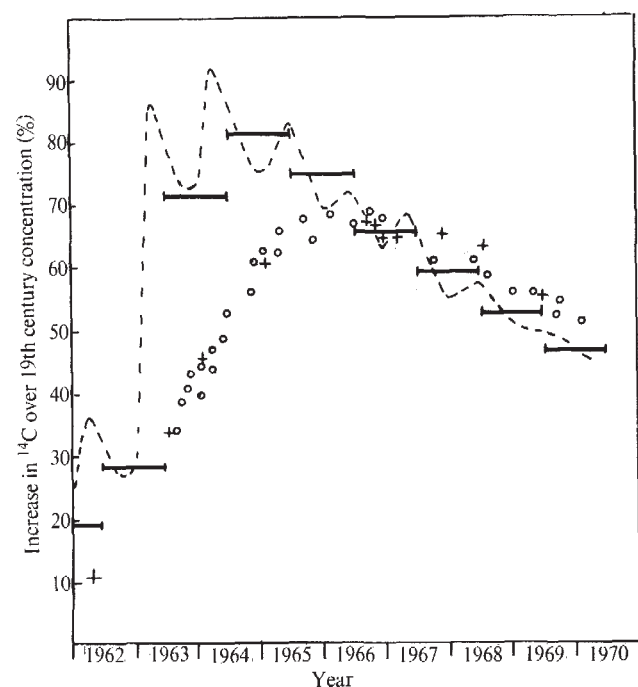


Fig. 1 Variations in ^{14}C concentrations at UK latitudes. ---, Tropospheric carbon dioxide. —, Computed values for UK diet (D_t). —, Whole blood¹⁵. +, Blood protein¹⁴.

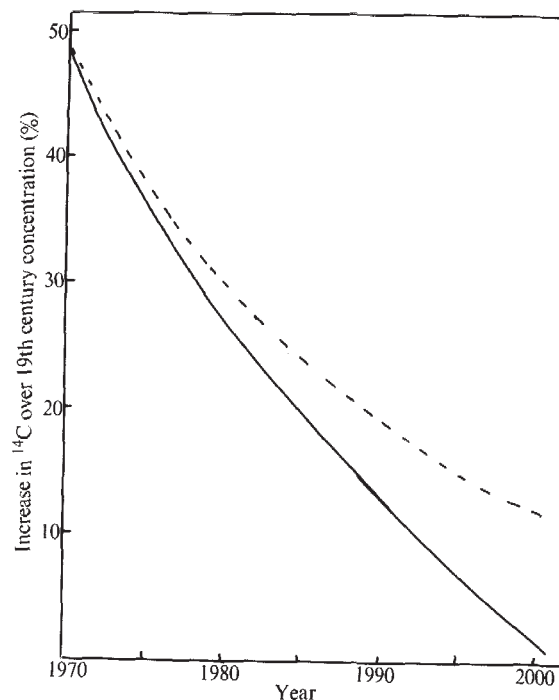


Fig. 2 Predicted atmospheric ^{14}C concentrations after 1970. ---, Decrease due to atmosphere/ocean exchange. —, Total decrease (exchange plus Suess effect).

of A_t were selected to coincide with the annual growth period for UK crops. Values of D_t were calculated for July of each year. The computed concentrations of ^{14}C in the diet (D_t) with those measured in human blood^{14,15} are compared in Fig. 1. The comparison shows that discrepancies between contemporary concentrations of "bomb ^{14}C " in the atmosphere and in human tissue cannot be attributed entirely to dietary modification. It seems that the finite turnover time for carbon in the human body depends on ^{14}C concentrations in the environment.

Are the temporal variations reported for ^{14}C in blood carbon representative of all body tissue? Variations in the metabolic renewal rates among various tissues, if sufficiently large, will result in a non-uniform distribution of the total body burden of "bomb ^{14}C ".

The distribution of ^{14}C as monitored in the tissues of a female resident of Glasgow, who died at the age of 37 of coronary artery disease in September 1969, is presented in Table 2. The marked variation noted between ^{14}C concentrations in the skeleton and in soft tissue can only result from the carbon in bone structures being several years older than that in the remainder of the body. Smaller variations in the ^{14}C concentrations in individual soft tissues disappear with the statistical uncertainty associated with ^{14}C assay. More data are necessary to confirm such trends and to allow their interpretation in terms of tissue renewal rates.

Table 1 $^{14}\text{C}/^{12}\text{C}$ Ratios Measured in UK Dietary Components

Food	Collection date	$\delta^{14}\text{C}\%$ $\pm 1\sigma$	$\delta^{13}\text{C}$ ‰	$\Delta\%$ $\pm 1\sigma$
Milk	July 1967	62.9 ± 0.8	-23.7	62.4 ± 0.9
Milk	Sept. 1967	60.9 ± 0.8	-24.2	60.8 ± 0.8
Beef	Aug. 1967	59.2 ± 0.7	-27.2	59.7 ± 0.8
Milk	Aug. 1968	55.3 ± 1.2	-23.7	54.9 ± 1.2
Potatoes	Sept. 1968	55.4 ± 1.1	-23.6	54.9 ± 1.1
Beef	Aug. 1968	56.8 ± 0.8	-26.0	57.0 ± 0.9
Fish (cod)	Dec. 1968	7.0 ± 0.4	-24.4	6.8 ± 0.5
Fish (whiting)	Dec. 1968	10.8 ± 0.6	-22.1	10.2 ± 0.7
Fish (skate)	Dec. 1968	1.6 ± 0.5	-23.5	1.3 ± 0.6
Cheese	July 1969	52.6 ± 1.2	-24.0	52.5 ± 1.3
Beef	Nov. 1969	54.1 ± 0.7	-33.1	56.5 ± 0.8

^{14}C enrichment values ($\Delta\%$) relate to the NBS oxalic acid standard and were calculated using the methods of Broecker and Olson²⁰.

Table 2 Distribution of ^{14}C in the Human Body

Tissue	$\delta^{14}\text{C}\text{‰} \pm 1\sigma$	$\delta^{13}\text{C}\text{‰}$	$\Delta\text{‰} \pm 1\sigma$
Brain	58.3 ± 0.7	-22.4	57.5 ± 0.8
Liver	50.3 ± 0.8	-27.9	51.2 ± 0.8
Kidney	52.9 ± 0.7	-20.5	51.5 ± 0.8
Uterus	45.9 ± 0.7	-27.4	46.7 ± 0.7
Ovaries	53.5 ± 1.3	-28.2	54.5 ± 1.4
Muscle	55.5 ± 0.7	-26.6	56.0 ± 0.8
Fat	48.2 ± 0.8	-30.4	49.8 ± 0.8
Bone marrow	41.3 ± 0.8	-34.2	43.9 ± 0.8
Bone collagen	25.7 ± 0.7	-27.6	26.4 ± 0.8
Bone mineral	22.4 ± 0.7	-17.1	20.5 ± 0.8

During the remainder of this century, in the absence of any further release of "artificial ^{14}C ", the present uniform concentration of ^{14}C in the atmosphere will decrease almost exponentially. The rate of decrease is determined by two mechanisms; first, the equilibration of atmospheric and oceanic carbon and, second, the release of " ^{14}C free" carbon dioxide to the atmosphere by the combustion of fossil fuels (Suess effect). We have assessed the trend in atmospheric ^{14}C concentrations after 1970 (Fig. 2) on the basis of values of carbon exchange rates and predicted increases in the Suess effect^{1,19}.

It is anticipated that concentrations of ^{14}C in living structures will parallel the steady atmospheric decline. Future predicted conditions would appear ideal for the advance of biogenic studies which involve "bomb ^{14}C " as a kinetic tracer.

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Recognition of Shapes across Modalities by Infants

HUMAN adults and children as young as four years can recognize two identical objects as equivalent when they touch one and see the other¹⁻³. It is not yet known what mechanisms underlie this cross-modal ability. One suggestion⁴⁻⁶ has been that humans are able to match identical objects cross-modally

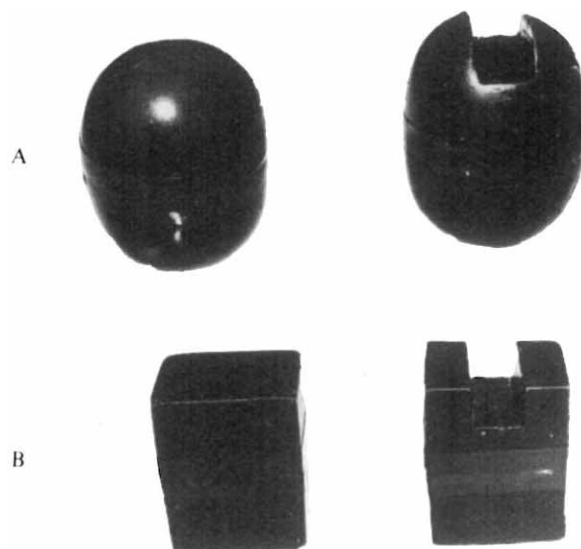


Fig. 1 The two pairs of shapes.

because they can give the visual and tactual objects the same name. This hypothesis has recently been questioned⁷, and an experiment which showed that chimpanzees and orang-outangs can to some extent match shapes cross-modally⁸ makes it improbable that cross-modal mechanisms need always be based on language. But it may still be true that humans have to be able to attach common verbal labels to visual and tactual inputs in order to treat them as the same.

If this hypothesis were correct, human infants, who have no language, would have no cross-modal ability. We have recently completed two experiments which demonstrate that infants can recognize some objects cross-modally before they learn to speak.

We used objects which could emit sounds. Having observed that infants are attracted to, and reach for, objects which have made a noise, we decided to present an object tactually, at the same time making it produce a sound, and then to present it again visually, but this time paired with another object of a different shape. We argued that if most infants then reached for the object which they had touched and which had made a noise, they must do so by recognizing the tactual and visual versions of the same shape as equivalent.

Our material consisted of two pairs of three-dimensional shapes (Fig. 1), made out of fibre glass. They were hollow, 5 cm high and 3.5 cm wide. Inside was a bleeper activated by a mercury switch when the object was slightly tilted. Each pair involved a complete and an incomplete shape, Pair A being an ellipsoid and an incomplete ellipsoid with a ridge 1 cm wide and 1 cm deep, and Pair B a cuboid and a similarly incomplete cuboid. Each infant was given one of these pairs. We used such pairs because previous work has shown that older children find discriminations between complete and incomplete shapes relatively easy in a cross-modal task^{9,10}.

In the first experiment we saw 120 infants, 60 of whom (mean age 8.7 months: range 6-11.5) were given a cross-modal discrimination and 60 (mean age 8.4 months: range 6-11) a within-modal one. We also excluded eighteen infants who never made a reaching response at all. The infant sat on his mother's lap in front of a table during the session, which lasted about 5 min. The session involved three successive stages. The first consisted of a visual presentation without choice. The two objects were put on the table out of the infant's reach and, when the infant had looked at both, they were removed from his sight. The second consisted of a tactual presentation with noise. The experimenter then took one of the objects, completely concealed in one of her hands, over to the infant. With her other hand she took one of the infant's hands and placed it on the shape, at the same time encircling the infant's hands with her own hands. Neither the infant nor the mother

Table 1 Numbers of Infants who Reached for the Object Previously Presented with Noise, and who Reached for the Other Object

		Total No. of infants reaching	Pair A	Total No. of infants reaching	Pair B	Total No. of infants reaching
			No. reaching for previously presented object		No. reaching for previously presented object	
Experiment 1 (single experi- menters)	Cross-modal condition	30	23	7	30	19
	Within-modal condition	30	21	9	30	22
Experiment 2 (two experi- menters, blind procedure)	Cross-modal condition	31	24	7	30	18
						12

could see it at any time during this stage. While the infant's hand was on the object the experimenter made it produce a noise and this was the only time when the object made any sound. It was then removed. An important point is that the experimenter did her best to encourage the child's attention by talking to him through this stage. The third stage consisted of a visual presentation with choice. Both objects were put back on the table, at first out of the infant's reach; when he had looked at them, they were pushed forward within his reach, and the experimenter judged which object the infant's hand first contacted.

In the within-modal condition the first and third stages were exactly the same. In stage 2, however, the presentation was visual, and the single object was put on the table and made to produce a sound. In both conditions half of the infants were given Pair A and half Pair B. With both pairs half of the infants were given one of the two objects in stage 2 and half the other. We varied the left-right positioning of the two objects in each pair in stages 1 and 3 between children.

Table 1 shows how the children reached in stage 3 of the first experiment. In the cross-modal condition the majority did reach for the object which they had previously touched, and this was true for both pairs. The number reaching for the previously felt object was significantly greater than could be expected by chance in the case of Pair A ($P < 0.006$), but not in the case of Pair B ($P < 0.137$). In the within-modal condition, on the other hand, the number reaching for the object previously presented with noise was significantly above chance both with Pair A ($P < 0.014$) and with Pair B ($P < 0.020$). Thus the first experiment confirmed that noisy objects attract infants and demonstrated that infants can, under some circumstances, recognize objects cross-modally.

The procedure involved only one experimenter, however. It is possible that the experimenter might unwittingly have influenced the infant's choice by the way she placed the two objects on the table in stage 3. We therefore decided to repeat the cross-modal discriminations with two experimenters, one of whom handled the visual presentations (stages 1 and 3), the other the tactual presentation (stage 2). The former experimenter never knew which of the two was felt by the infant in stage 2. The latter experimenter kept out of the infant's sight when he made his choice in stage 3. Otherwise the procedure was the same as in the first experiment.

Sixty-one infants took part in this experiment (mean age 8.6 months: range 6–11), thirty-one of whom were given Pair A and thirty Pair B. We also excluded fourteen infants because they did not reach in stage 3. Table 1 shows that more infants than could be expected by chance reached for the previously felt object with Pair A ($P < 0.003$) but not with Pair B ($P < 0.138$). Thus this experiment confirmed the results of the first experiment.

Finally, in a third experiment with 30 infants (mean age 8.7 months, range 6–11.7), two different experimenters replicated the successful cross-modal discriminations of Pair A. Twenty-one of the infants reached for the previously felt object ($P < 0.014$).

Two issues are raised by these results, the first concerning the infants' failure with Pair B and the second their success with Pair A, in the cross-modal condition. Their performance with Pair B in the visual within-modal condition shows that

their apparent failure with this pair in the cross-modal condition was not a visual failure. But we do not know whether the difficulty caused by this pair was due to a tactual failure or to a failure to relate visual to tactual information. The answer to this question can only come with the use of a tactual, within-modal task¹¹. One can still make a tentative suggestion about the nature of the features which cause the failure. One possibility is that both objects in this pair had only straight contours, and that a straight-straight discrimination is a very difficult one either tactually or cross-modally.

The consistent success of infants younger than 12 months in discriminating the two objects in Pair A cross-modally has two implications for theories of cross-modal integration. First, it demonstrates that humans can transmit some information about shape across modalities without the benefit of words. Second, it shows that cross-modal mechanisms, though not necessarily innate, are present at an early stage in perceptual development.

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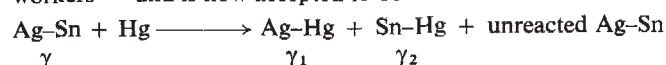
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Corrosion of Dental Amalgam

DENTAL amalgam is incorporated into about 80% of all dental restorations. It is prepared by grinding or triturating a silver-tin alloy (Ag_3Sn , the γ phase) with mercury in the proportions 1 : 1 or 5 : 6 of alloy to mercury. The setting reaction of dental amalgam has been studied by many workers¹⁻⁴ and is now accepted to be



Excess mercury is removed from the amalgam both before and during insertion into the cavity. There is, however, some controversy over whether the γ_2 phase is continuous throughout the matrix⁵ or whether it is present in discrete clumps or clusters (G. Wing, personal communication).

Many workers have investigated amalgam corrosion⁶⁻¹³, but the detailed mechanism is still in doubt. We have proposed a mechanism¹⁵ for amalgam corrosion but no experimental data were presented to substantiate it. The basic concept of this mechanism was that the tin-rich (γ_2) phase of the amalgam functions as a sacrificial anode in a galvanic cell. Here we report confirmation of this corrosion theory of amalgam by means of a zero-resistance ammeter technique¹⁵.

A schematic representation of the experimental circuit is given in Fig. 1; the circuit involved the use of a potentiostat set to a control potential of 0.00 V so that it functioned as a zero resistance ammeter¹⁶. The test electrodes were (a) a composite set dental amalgam, that is, a mixture of γ_1 (Ag_2Hg_3) and γ_2 (Sn_7Hg) prepared by reacting stoichiometric quantities of mercury with the pure (Analar grade) metal powders, and (b) composite γ_1 , prepared from stoichiometric proportions of

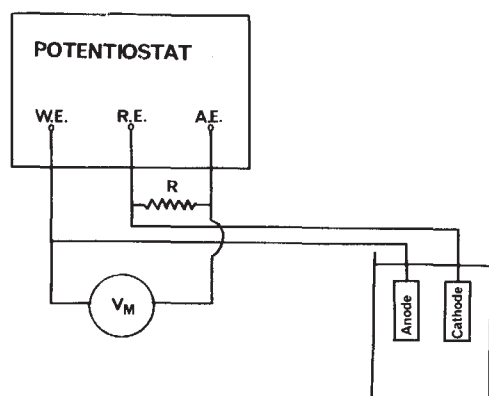


Fig. 1 Schematic diagram of potentiostat and corrosion cell.

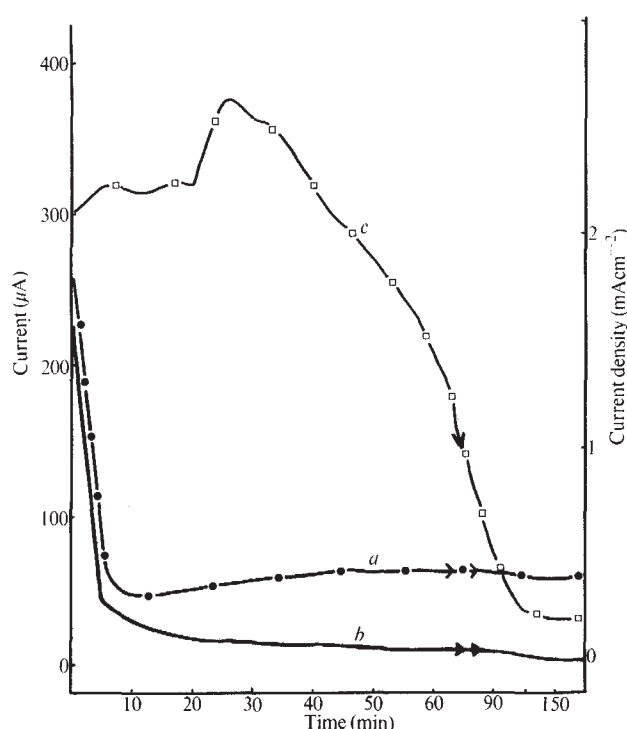


Fig. 2 Current/time behaviour of: a, Composite set amalgam ($\gamma_1 + \gamma_2$) against silver-rich γ_1 phase. b, Tin-rich γ_2 phase against mercury. c, Silver-rich γ_1 phase against mercury. The electrolyte was 1% KCl solution, pH 6.2.

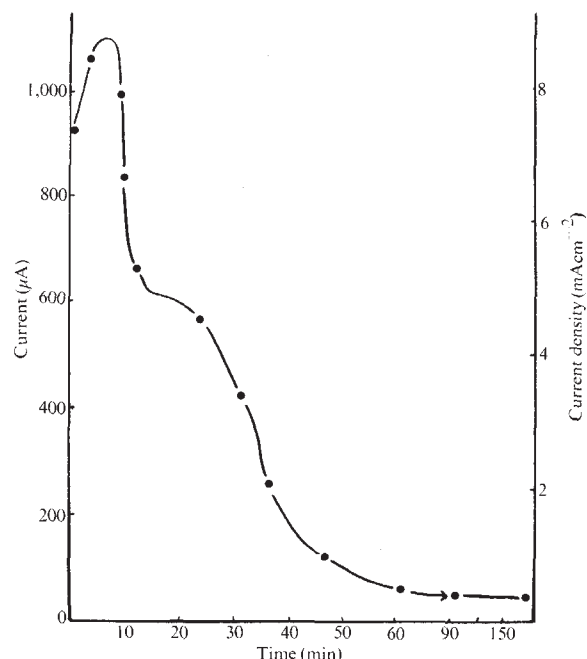


Fig. 3 Silver-rich γ_1 phase and tin-rich γ_2 phase. Electrolyte as Fig. 2.

mercury and silver mounted in acrylic blocks with insulated copper wires as external connexions. The initial open-circuit potentials of the two electrodes were typically -520 mV, SCE (set amalgam) and -400 mV, SCE (γ_1); the set amalgam containing the γ_2 phase was about 120 mV more active. To simplify this study, 1% KCl solution was used as the electrolyte because natural saliva is subject to large variations in pH, conductivity and organic matter content. Furthermore, there are many problems associated with collection of a representative sample of saliva¹⁶. Many synthetic saliva solutions have been proposed, but these differ little in conductivity and pH from 1% KCl solution. This is rather more aggressive than natural saliva, but whilst this will accelerate the corrosion reactions, it should not alter the basic mechanism.

The two electrodes, of equal surface area, were connected as a galvanic couple in 1% KCl, pH 6.2, through the zero resistance ammeter. The change in corrosion current in the galvanic cell is given in Fig. 2a, a typical current-time plot for this cell. It can be seen that a very high initial current flows between the anodic $\gamma_1 + \gamma_2$ amalgam electrode and the noble (cathodic) γ_1 electrode. This current decays after some 5–10 min to a low steady-state value. At this stage, both the anode and the cathode of the cell are polarized, the open-circuit potential of the $\gamma_1 + \gamma_2$ amalgam electrode being typically -480 mV, SCE and that of the γ_1 electrode -440 mV, SCE.

Galvanic coupling of electrodes, consisting of the tin-rich (γ_2) phase and mercury (Fig. 2b), exhibits similar behaviour with polarization of the corrosion reaction becoming established within 4–5 min. Coupling of the silver-rich (γ_1) phase to mercury (Fig. 2c), however, differs in that the reaction is far more rapid and the γ_1 phase is anodic to mercury. Polarization is only established after some 90 min.

The most pronounced reaction, and presumably the most favoured, is found when the γ_1 phase is coupled to γ_2 , the latter being anodic (Fig. 3).

In summary, the γ_2 phase in dental amalgam is preferentially corroded, with mercury and the silver-rich γ_1 phase functioning as cathodes. Mercury released by dissolution of tin from the γ_2 phase may react with residual γ (Ag_3Sn) in the matrix or with γ_1 phase. The former reaction would result in further γ_1 and γ_2 phase formation, whilst the latter reaction would form a mercury-rich phase, cathodic to γ_1 , although the reaction is very slow and rapidly polarizes.

Our mechanism for amalgam corrosion¹⁴ is thus confirmed, as is the theory that mercury in dental amalgam restorations

does not corrode. There would appear to be no toxicity hazard for the patient from dental amalgam restorations. In fact the rapid rate of polarization of the different corrosion reactions in dental amalgam suggests that corrosion is somewhat beneficial in that the marginal gap between the restoration and tooth is sealed by the formation of corrosion products of limited solubility.

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Dutch Elm Disease—Recurrence and Recovery in Britain

SURVEYS¹ of Dutch elm disease in Britain between 1928 and 1955 suggested that native elms attacked by the disease very frequently recovered, even when a large part of the crown was

these, no evidence of new beetle attack could be found and disease recurrence was thought probable.

Assessments in July 1972 on various elm species inoculated in June 1971 with aggressive and non-aggressive isolates of *C. ulmi* yielded further information on recurrence. All inoculated elms showed symptoms of the disease in 1971; those inoculated with the aggressive strain showed the more severe symptoms. No scolytid beetles were present as vectors of the disease in either 1971 or 1972. The results of assessment of disease recurrence in 1972 are shown in Table 1.

The disease has recurred in all species of elm tested (*U. glabra*, *U. carpinifolia* and *U. americana*), and of the sixteen cases of recurrence, fifteen occurred with the aggressive strain. The one case of recurrence with the non-aggressive strain may have been aided by knife cuts made into the tree early in 1972, allowing the fungus to pass from one annual ring to the next.

Preliminary histological examination of stem sections from the inoculated trees showed that staining of the vascular tissue extended at least to the root collar with both strains of the fungus. A greater proportion of the 1971 annual ring was affected by the aggressive strain, which was much narrower as a result of more extensive crown dieback. The chances of cross-over from the 1971 to the 1972 annual ring may well have been greater with the aggressive strain. The exact location of the cross-over in these trees has yet to be determined. The passage of the disease from one year to the next within the tree provides a means of carry-over in the absence of the beetle. This may allow an epidemic to continue even in seasons of low insect activity.

In Britain recurrence of the disease after initial attack may have become more frequent in recent years, and this may be associated with the presence of the aggressive strain. This suggestion is supported by data from the 1971 Forestry Commission Survey⁴ which showed that there was little sign of recovery from the disease apparent in the severe outbreak areas.

These observations may have considerable significance in the future development of the epidemic and in measures taken towards its control. In the present sanitation felling programme it is recommended that those trees with more than half the crown affected by the disease should be felled and the bark

Table 1 Assessment of Recurrence of Dutch Elm Disease in Various Elm Species

Species	Age in 1972	Strain of <i>C. ulmi</i>	No. of trees inoculated June 1971	No. of trees alive July 1972	No. of trees with recurrence July 1972
<i>U. glabra</i> (seedlings)	5 yr	Aggressive	35	31	8
		Non-aggressive	35	35	1
<i>U. carpinifolia</i> (Cornish elm) (rooted cuttings)	6 yr	Aggressive	7	7	4
		Non-aggressive	7	7	0
<i>U. carpinifolia</i> (Wheatley elm) (rooted cuttings)	6 yr	Aggressive	3	2	1
		Non-aggressive	3	3	0
<i>U. americana</i> (seedlings)	4 yr	Aggressive	40	15	2
		Non-aggressive	40	32	0

affected, contrary to experience elsewhere. In the United States², for example, more than 90% of American elms that were diseased throughout the crown and were not killed in the first year died subsequently as a result of the passage of the fungus (*Ceratocystis ulmi*) from one annual ring to the next, either in the root system and lower trunk or possibly in the crown, and not because of reinfection by the insect vectors. Following the report of the presence of an unusually aggressive strain of *C. ulmi* in the current severe outbreak areas in Britain³, we have set out to test whether the earlier conclusions¹ about recovery in native elms are still valid.

A study of young English elm (*Ulmus procera*) about 5 yr old and 2–3 m in height was made in early June 1972, before the main period of beetle emergence, in an area (Frensham, Surrey) where aggressive isolates of the fungus had earlier been obtained. Eleven trees, infected in the previous year and showing fresh foliar symptoms, were examined. In ten of

destroyed. This recommendation is based on two major points: first, only severely affected trees act as a major breeding site for the beetle; and second, there is a good chance that less severely affected trees will recover.

The first reason remains valid but the second may now be questioned at least in the severe outbreak areas where the aggressive strain is prevalent.

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CORRESPONDENCE

NATO and Science

SIR,—I should like to make some points relevant to the controversy about NATO support for science.

NATO is not a source of wealth; the money it distributes for science comes from the member governments. The amount NATO has to spend on science is decided not by the organization itself but, ultimately, by the North Atlantic Council, made up of representatives of the member governments. If NATO did not have a science programme these governments would contribute less to NATO. The bulk of NATO's science spending goes on its scientific fellowships. Here not only is the money provided by the member governments, but also the money is returned to national agencies which administer its distribution. In Britain, for example, the agency is the SRC and in the United States the NSF¹. Thus almost the only effect of NATO participation in this area is to add to the overhead costs and to attach NATO's name to grants provided by the member governments. Would it not be better if the money was given directly to the various non-military national agencies concerned?

Two points made by Mr Kovach (*Nature*, **239**, 476; 1972) about the NATO Science Programme require some addition. First, he correctly stated that people from non-NATO countries are free to attend NATO Advanced Study Institutes. However, the leaflet which advertises the Advanced Study Institute Programme makes it clear that "... NATO funds cannot be applied directly to meet the expenses of non-NATO participants". Secondly, I should like to add to the list of Advanced Study Institutes mentioned by Mr Kovach the following conferences, which, while not Advanced Study Institutes, were held under the aegis of the NATO Science Committee and were sponsored by two of its advisory panels: Human Factors in the Design of Weapons Systems, Soesterberg, 1961; Military Logistics, The Hague, 1962; Military Applications of Programmed Learning, Naples, 1965; The Operational Evaluation of ASW Weapons Systems, Paris, 1966¹.

I have no space to discuss in detail the motives behind the NATO science programme. However, it is worth remarking that NATO has always seen the programme as related to its military and political aims. The "philosophy"

behind the programme has been described as the belief that "national and international strength are related directly to progress in science and technology, the basis of economic and military development"¹ and it has been explained that under the work of the Science Committee "science and technology have been analysed continuously as factors helping to guarantee the success of the alliance... engagement in scientific affairs gives NATO values other than purely military ones but ones which are essential in maintaining the strength and the cohesion of the alliance."¹ No reminder should be necessary that the "success of the alliance" has involved support for Portugal's colonial wars in Africa² and, as well as the military dictatorship in Portugal, the more recent military dictatorship in Greece³.

Scientists who oppose the military and political aims of NATO, who do not wish to add to NATO's prestige, or who are against military involvement in science, should refuse to collaborate with the NATO science programme. Instead they should press that the money now given to NATO to spend on science should be given instead to national or international agencies (such as UNESCO) of a non-military nature.

Yours faithfully,

ALAN SLOMSON

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¹ *NATO and Science*, 142 (NATO, Brussels, 1967).

² Bosgra, S. J., and van Krimpen, C., *Portugal and NATO*, 48 (Angola Comite, Amsterdam, 1969).

³ Wilmers, G., in *Proc. Bertrand Russell Memorial Logic Conference* (in the press).

Peptide Synthesis

SIR,—Your Molecular Biology Correspondent has added to our gaiety on many occasions, and perhaps his opening statement (*Nature*, **239**, 252; 1972) that "the evolution of solid state techniques has transformed the craft of peptide synthesis into a relatively routine, or at least not too intimidating operation" was intended in that spirit. It does, however, contrast with the authoritative pronouncement by E. Wunsch (*Angewandte Chemie Int. Edn.*, **10**, 791; 1971) that "the Merrifield tech-

nique in its present form is unsuitable for the satisfactory synthesis of higher natural peptides (with more than 15 amino-acid residues)". That was the tenor of discussions at the Third American Peptide Symposium, Boston, June 1972, and the Twelfth European Peptide Symposium, Reinhardtsbrunn, September 1972. Can your correspondent quote a single example of a peptide with twenty or more amino-acid residues which has been synthesized by a solid-phase technique to the normal standards of chemistry?

This is not a merely rhetorical question concerning a point for specialists. The real question is whether molecular biology is to operate at the levels of precision of molecular science or biological science.

Yours faithfully,

G. W. KENNER

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PO Box 147,
Liverpool L69 3BX*

Our Molecular Biology Correspondent replies:

If I have brought gaiety to Liverpool, this is sufficient reward. At the same time I confess that it is at least my aim to operate at the level of precision of molecular science. I would not presume to argue with Professor Kenner about peptide chemistry, but I wonder whether the point of his letter has eluded me. In the first place the work that I discussed in the article to which he takes exception concerned the synthesis of peptides of fourteen residues and no more, to which, he seems to agree, that the solid-state method is applicable. Second, had he quoted my second sentence, and not only the first, he would have come to the *caveat* about the difficulty of synthesizing long chains. As to his challenge to cite examples of the synthesis of longer peptides than twenty residues, two should suffice: Merrifield synthesized functional ribonuclease (124 residues), and Anfinsen a forty-two residue tract of the staphylococcal nuclease chain. It is true that affinity methods were used to separate correct from incorrect sequences, and perhaps to the true peptide chemist this is not cricket. The fact, nevertheless, as it seems to me, is that the solid-phase technique has for the first time brought at least the more modest essays in peptide synthesis into the range of a great many laboratories,

and, as the literature of protein chemistry shows, has been used to good effect. The difficulties have been widely enough discussed and publicized. If Professor Kenner feels that the practical advantages of the solid-phase method are all a great delusion, then I am sure no one will be listened to with greater interest.

Economics and Ecology

SIR,—Of recent years it has seemed that the needs of our environment, as seen by ecologists, and the needs of process technologists, epitomized by the laws of economics, were irrevocably opposed. We need to seek any means for moderating this conflict. One of the contentious issues is the limited term of years for which patents run. It is in the early years that a manufacturer may make most profit from a valuable patented process. Moreover, the operative term of years runs from the sealing date. The manufacturer is unlikely willingly to accept the advice of an ecologist that several years may be needed to evaluate whether by-products from working a patent will or will not harm the environment.

After three months or so an application for a patent may be granted. If, then, the patentee does not arrange for the use of the process which is the subject of the patent the monopoly rights may be lost. No provision is made for delay while ecological studies assess whether working the patent may damage the environment. Notoriously ecologists find it difficult to give straight answers to straight questions posed by technologists, but the more the time that is available the easier it is to arrive at a straightforward answer. Since three years is probably a minimum time to obtain sufficient information to attempt reasonably confident judgments, I suggest as a compromise that the needs of environmental protection would be furthered if the operational date for a patent covering a chemical process could be deferred for, say, three years after the sealing or granting date.

The decision as to whether such ecological evaluation is necessary would be made by Patent Office examiners, acting on the advice of environmental ecologists on the staff of governmental laboratories such as those of the Ministry of Agriculture, Fisheries and Food or the Water Pollution Research Laboratory.

The necessary assessment of the environmental consequences of newly patented processes could be referred to an ecological laboratory which might well develop under the wing of the Government Chemist but be largely financed by fees from firms soliciting

patent protection for new processes. This laboratory would assess the environmental hazards inherent in new processes submitted for Letters Patent. Its reports could include statements of the kinds of treatment needed to render harmful effluents harmless and would be submitted back to the Patent Office whose examiners would have acquired the experience to check, evaluate and implement the ecological findings.

A process patent, tentatively sealed under the present patent laws, would become operational only on the date of approval by the ecological examiners and with any operational restrictions which they might think fit to impose.

During the period of evaluation the manufacturer would be protected from pirating of his process and would then have the full term of years in which to exploit his patent. The environment would be protected much more than it now is. Economics and ecology would learn to live together.

Any country which unilaterally adopted such a development of Patent Law would place its manufacturers at a disadvantage compared with manufacturers which did not have to work within such a law. Clearly international agreement would be needed before any country could enforce such a law.

There may be circumstances in which a manufacturer, introducing a new process which he knew could provide a hazard to the environment, would circumvent the law by the simple expedient of not applying for patent protection. To meet this situation the law would need to contain the provision that a new emission to the environment of any substance or form of energy is presumed to be hazardous to the environment until it has been proved not to be so to the satisfaction of the appropriate Inspecting Officer or Patent Examiner.

To sum up, wise international development of the Patent Laws could provide effective control of adverse effects of new technological emissions upon the environment.

Yours faithfully,

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Insect Taxonomy

SIR,—It is not only in Britain that insect taxonomy is regrettably so neglected (*Nature*, 238, 475; 1972). The same situation obtains in many other countries. I am told by colleagues abroad, and am a witness to the problem myself, that the always notoriously limited financial and moral support for this field of enquiry is in fact decreasing

yearly. In this context, the following points may prove of some interest.

(1) Registration of existing species, and arranging them into a system which would make possible reasonable orientation in an immense insect diversity, species identification, and suitable classification of any kind of information on insects—these are the primary goals of insect taxonomists. But because of the imbalance between the extent of the group (some 1,000,000 species recognized and several times as many still unknown) and the limited number of taxonomists on a world-wide scale, the field still suffers from substantial *lacunae* even in the most basic descriptive data.

(2) A vast amount of work has been done and is still being done by amateur entomologists. The result is that the layman cannot always understand how severely insect taxonomy is understaffed. Moreover, factors such as rising expenses of all kinds and the growing necessity for entomologists to gain a broader biological background logically render it difficult for amateurs to keep in step with the professionals.

(3) Insect taxonomists play a double role in society: they do research and they provide identification services. The importance of precise identification for applied biology, as well as for the standardization of biological material for further study, is generally recognized; but few people are aware how very frequently such services are required. Usually, reliable identification can only be undertaken by research taxonomists actively working in the pertinent groups of insects. Yet the world-wide shortage of experienced specialists frequently results in these services being provided by less qualified persons, with a consequent high percentage of misidentifications or nomenclatorial inaccuracies. Chronic scarcity of identification manuals, catalogues, cumulative faunal lists, and similar reference works is another consequence of this state of affairs.

(4) It is still possible to achieve serious and often outstanding scientific advances in insect taxonomy by the use of simple methods and with minimum expense. It is absurd to consider this a shortcoming or a reason for reduced monetary support; it would be equally ludicrous to look down on insect taxonomy as an obsolete science where "everything has already been done" simply because it has been intensively pursued for more than two centuries. It cannot be stressed too emphatically that the taxonomic field necessitates not only extensive development of basic, descriptive and registratory work, but also substantial advances at the higher, synthetic and causal levels. This requires an interdisciplinary approach and consequently the use of varied

experimental techniques, numerical methods, electron microscopy, etc. But projects of this kind are very far from achieving reality without funds comparable with those afforded to other, more "modern" branches of biology.

Insect taxonomists have contributed substantially to the advancement of science as well as to everyday scientific practice. Most of the research work, however, still remains to be done, while it is also imperative to ensure the existence of reliable identification services. Both are the basic responsibility of taxonomists toward society. On the other hand, I am convinced it is the moral duty of science administrators to grant enough support for insect taxonomy to allow it to develop as progressively and successfully as other fields of biological enquiry.

Yours faithfully,

JAN ZUSKA

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Faulty Flying Machine

SIR,—I believe your readers would be concerned to hear of the unfortunate fate of a colleague who constructed a flying machine based upon a design of H. C. Bennet-Clark (*Nature*, **239**, 451; 1972). The machine became airborne satisfactorily but when adjusting the trim to change from a subsonic to a supersonic condition it veered uncontrollably and, sadly to say, fatally to starboard.

Subsequent examination of the black box flight recorder yielded the last message from my colleague, couched in understandably uninhibited terms, to the effect that "... the ... dotted line on the starboard fin should have been drawn through the ... corner".

Yours faithfully,

D. C. SPOONER

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IQ Absurdity

SIR,—There appears to be an absurd recommendation towards the end of the first paragraph of "How much of IQ is inherited?" (*Nature*, **240**, 69; 1972). You say, in effect, that, even where valid conclusions are being drawn from accurate observation of a phenomenon, policy makers should be absolved from paying them any attention. Using a closely similar example, may I point out the unsoundness of this notion?

It is the generally accepted view among psychologists that there is no inherent difference between the sexes in the distribution of IQ. I conclude, from your comments, that you would consider it entirely proper for this fact to be disregarded by social policy makers, on the grounds that it is likely to be irrelevant to decisions about, for example, equal treatment for the sexes at work. If this should not be a permissible conclusion for me to draw, I would welcome your explanation of the critical ways in which it differs logically from the argument which you have presented.

The emotional content of racial questions has a tendency to cloud real issues. I would like to suggest that the problem to which educational psychologists, social policy makers, and the editors of learned journals should be addressing themselves is that of providing suitable educational facilities for as wide a range of intellectual ability as possible. In this way, most people would receive treatment tailored to their special needs, and the colour of their skin would be seen for the red herring it is.

Yours faithfully,

ANDREW M. STEWART

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Law of the Sea

SIR,—The article on "Threat to Deep Sea Research" (*Nature*, **239**, 421; 1972) is misleading when it refers to permission usually being granted for work on the continental shelf. For example, Turkey insists that national organizations should participate. However, even when this condition is fulfilled the Turkish Government consistently refuses permission on security grounds. Similarly Greece usually insists that a naval observer is on board, but when this is welcomed and national organizations are participating by invitation, refuses permission for magnetic, gravity and bathymetric surveys.

These are two countries selected from many examples which could be quoted, but several other countries both in Europe and elsewhere are becoming increasingly restrictive, generally on "security" grounds. It is always easier to refuse permission than to grant it.

Yours faithfully,

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